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ART. XVI.—*Comparison of the mean daily range of the Magnetic Declination, with the number of Auroras observed each year, and the extent of the black Spots on the surface of the Sun;* by ELIAS LOOMIS, Professor of Natural Philosophy in Yale College.

IN 1826, M. Schwabe of Dessau in Germany commenced a series of observations of the solar spots which he has continued to the present time. For each year he has kept a record of the number of days of observation, the number of groups of spots observed, and the days on which the sun was free from spots. These observations decidedly indicate a periodicity in the number of the solar spots, a maximum recurring at an interval of from 7 to 13 years. In 1849, Dr. Rudolf Wolf, of Zurich, Switzerland, commenced a series of observations for the same object as those of Schwabe, but upon a plan somewhat more precise and thorough. For each day of observation he recorded two numbers; the first showed how many groups or isolated spots were seen with a four-feet Fraunhofer telescope and a magnifying power of 64; the second showed the total number of visible spots for that day. In order to deduce from these observations a series of numbers which shall be proportional to the amount of spotted surface of the sun, he multiplies the number of groups for each day by ten, and adds to this product the total number of spots. Thus, if on a certain day he counts 9 groups and 31 single spots, he obtains 121 ($9\times10+31$) which he calls the relative number. The mean of all the numbers thus obtained for a month, is the relative number for that month; and the mean of the number thus obtained for

a year is the relative number for that year. Dr. Wolf has continued these observations to the present time, and they show clearly a minimum in 1856 and another in 1867, the interval being 11 years.

Dr. Wolf has made a most thorough examination of the records of Astronomical observers, since the invention of the telescope in 1608, and has aimed to deduce a similar series of numbers which shall be proportional to the amount of spotted surface of the sun for a period of 260 years. In this attempt he has been remarkably successful, and he claims to have determined the date of every maximum and every minimum of the solar spots since 1608. He has also published a table of relative numbers for each year since 1700, in which, however, we find 36 years marked with an interrogation point, indicating that the numbers for those years are specially unreliable, or were derived by interpolation. Since 1749 only 12 years are thus marked with an interrogation point. These years are 1774, 1792, 1793, 1801 to 1807, and 1814–5. From 1749 to 1825, the numbers for 37 years are claimed to be specially reliable.

The following is a list of all the important observations since 1700 from which these numbers are derived. All the references are to "Vierteljahrschrift der Naturforschenden Gesellschaft, in Zurich."

Obs. from 1700–1748 by Kirch, vol. 12, p. 142.
" 1705–1726 by Plantade, vol. 5, p. 258.
" 1718–1726 by Rost and Alischez, vol. 5, p. 261.
" 1742–1751 by Hagen, vol. 4, p. 250.
" 1749–1799 by Staudacher, vol. 2, p. 277.
" 1754–1760 by Zucconi, vol. 2, p. 285.
" 1769 by Horrebow, vol. 10, p. 281.
" 1773–1777 by Mallet, vol. 3, p. 394.
Obs. from 1794–1830 by Flaugergues, vol. 6, p. 433.
" 1800–1818 by Heinrich, vol. 4, p. 85.
" 1813–1836 by Stark, vol. 3, p. 373.
" 1816–1825 by Tevel, vol. 4, p. 239.
" 1819–1823 by Adams, vol. 6, p. 449.
" 1826–1868 by Schwabe, vol. 1, p. 266; vol. 5, p. 1, etc.
" 1849–1868 by Wolf, vols. 1, to 14.
" 1854–1860 by Carrington, vol. 9, p. 248.

I have no reason to doubt that the relative numbers which Dr. Wolf has derived from these observations are generally as accurate as can be deduced from the materials employed, but I think his numbers need some correction for the years 1793 and 1794 and probably also for 1795. His numbers from 1787 to 1795 are

1787	92·8*	1790	75·2	1793	20·7?
1788	90·6*	1791	46·1	1794	23·9
1789	85·4	1792 .	52·7?	1795	16·5

I propose to examine into the accuracy of these numbers for 1793 and 1794.

The relative numbers from 1787 to 1793 depend almost entirely upon the observations of Staudacher. On p. 283, vol. 2, Wolf, by comparing the average number of spots on each day of observation, deduces the following numbers :

1787.	1788.	1789.	1790.	1791.	1792.	1793.
46·4	45·3	39·4	37·6	22·8	21·7	17·0

The relative numbers which he gives for 1787 and 1788 are obtained by doubling the corresponding numbers here given for those years; and applying the same rule to 1793 we obtain 34 as the relative number for that year. If we combine with Staudacher's observations, the six observations made by Huber, Hahn, and Bode we shall obtain almost exactly the same result. I, therefore, adopt 34 as the most probable relative number for 1793.

The relative number for 1794 depends chiefly upon the observations of Flaugergues, and I will compare his observations for 1794 with those for 1816, for which year Wolf considers the relative number as well determined; and I will also include 15 days (Sept. 12 to 27, 1794) as being without spots. The following is a summary of the results:

	Groups.	Spots.	Days.	Average daily No. of Groups.	Spots.	Relative No.
1794,	41	148	35	1·17	4·23	15·9
1816,	62	112	51	1·22	2·20	14·4

The relative number which Wolf adopts for 1816 is 45·5. The above result for 1794, reduced to the same scale, becomes 50·2. If we combine with the observations of Flaugergues, observations on 15 days made by Ende, Herschel, Bode and Staudacher, this relative number will be somewhat increased. I therefore adopt 50 as a number not too great for the relative number of 1794, in place of 23·9 given by Wolf. A similar analysis applied to the observations of 1795 would give 35 as the correct relative number instead of 16·5. The maximum for 1804, Dr. Wolf estimates at 70; but this number is altogether conjectural, since observations are reported for only two days of that year. The observations of the magnetic declination indicate that this number is too great, and I have accordingly reduced it to 50, and have reduced the numbers for the three preceding and following years in the same ratio. The entire series of relative numbers from 1740 to 1868 will then be as follows. An asterisk (*) denotes a result considered specially trustworthy; an interrogation point (?) denotes a result considered specially untrustworthy.

Table of Relative Number of Solar Spots Each Year.

Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.	Year.	Number of Spots.
1740	60·0 ?	1766	17·5	1792	52·7 ?	1818	34 1*	1844	13·0*
1741	35·0 ?	1767	33·6	1793	34 0 ?	1819	22·5*	1845	33·0*
1742	18·3	1768	52·2	1794	50·0	1820	8·9*	1846	47·0*
1743	14·6	1769	85 7*	1795	35.0	1821	4·3*	1847	79·4*
1744	5·0 ?	1770	79·4*	1796	9·4*	1822	2·9*	1848	100·4*
1745	10·0 ?	1771	73·2*	1797	5·6*	1823	1 3*	1849	95·6*
1746	20·0 ?	1772	49·2	1798	2·8*	1824	6·7	1850	64·5*
1747	35·0 ?	1773	39·8	1799	5.9*	1825	17·4	1851	61·9*
1748	50·0 ?	1774	47·6 ?	1800	10·1*	1826	29·4*	1852	52·2*
1749	63·8*	1775	27·5	1801	22·1 ?	1827	39·9*	1853	37·7*
1750	68·2*	1776	35·2	1802	27·4 ?	1828	52·5	1854	19·2*
1751	40·9*	1777	63·0	1803	35·7 ?	1829	53·5*	1855	6·9*
1752	33·2*	1778	94·8	1804	50·0 ?	1830	59·1*	1856	4·2*
1753	23·1	1779	90 2	1805	35·7 ?	1831	38·8*	1857	21 6*
1754	13·8*	1780	72·6	1806	21·4 ?	1832	22·5*	1858	50·9*
1755	6·0*	1781	67·7	1807	7·1 ?	1833	7·5*	1859	96·4*
1756	8·8*	1782	33·2	1808	2·2	1834	11·4*	1860	98·6*
1757	30·4*	1783	22·5	1809	0·8	1835	45.5*	1861	77·4*
1758	38·3	1784	4·4	1810	0·0*	1836	96·7*	1862	59·4*
1759	48·6	1785	18·3	1811	0·9*	1837	111·0*	1863	44·4*
1760	48·9*	1786	60·8*	1812	5·4*	1838	82·6*	1864	47·1*
1761	75·0*	1787	92·8*	1813	13·7*	1839	68·5*	1865	32·5*
1762	50·6*	1788	90·6*	1814	20·0	1840	51·8*	1866	17·5*
1763	37·4*	1789	85·4	1815	35·0	1841	29·7*	1867	8·0*
1764	34·5*	1790	75·2	1816	45·5*	1842	19·5*	1868	40·2*
1765	23·0*	1791	46.1	1817	43·5*	1843	8·6*	1869	84·1*

The curve line at the bottom of the accompanying Plate, shows the fluctuations in this series of numbers, and exhibits a succession of maxima and minima which occur at the following dates:

Maxima.

Year.	Interval.	Year.	Interval.
1750		1804	10 years.
1761	11 years.	1816·5	12·5 "
1769	8 "	1830	13·5 "
1778	9 "	1837	7 "
1787	9 "	1848	11 "
1794	7 "	1860	12 "

Minima.

Year.	Interval.	Year.	Interval.
1744		1810	11·5 years.
1755·5	11·5 years.	1823	13 "
1766	10·5 "	1833	10 "
1775	9 "	1843	10 "
1784	9 "	1856	13 "
1793	9 "	1867	11 "
1798·5	5·5 "		

The maxima occur at intervals which vary from 7 to 13·5 years, the average interval being exactly ten years. The minima occur at intervals which vary from 5·5 to 13 years, the average interval being 10¼ years. Dr. Wolf overlooks the maximum of 1794 and counts but one period from 1787 to 1804. This interval amounts to 17 years, which is the same as the interval from the maximum of 1761 to that of 1778 (including two periods); and the interval from the maximum of 1830 to that of 1848 (including also two periods) is only 18 years. I think, therefore, we should count 1794 as a year of maximum, equal in amount to the following maxima of 1804 and 1816,

but almost blended with the preceding maximum of 1787, in consequence of an unusual prevalence of spots during the intermediate interval. From 1750 to 1860, Dr. Wolf reckons only 10 periods, which would make the average length of one period 11 years; but if we reckon 11 periods (as I think we ought) we shall find the average length of one period to be 10 years.

If now we attempt to explain the fluctuations in the sun's surface by ascribing them to the influence of the planets, we find that the planet whose period approaches nearest to the period of 10 or 11 years, is Jupiter, whose time of revolution is 11·86 years, which is nine months greater than Wolf's period, and almost two years greater than the period above found. But the interval between two successive heliocentric conjunctions of Jupiter and Saturn is 19·86 years. Once, therefore, in 9·93 years, Jupiter and Saturn are either in conjunction or opposition; and if we suppose that the action of these planets upon the sun has some analogy to that of the moon upon the earth in raising a tide, then we shall have a cause whose period corresponds quite accurately with the mean period of the maxima of the solar spots. It remains, however, to assign a cause why these periods are alternately increased and diminished by 3 or 4 years, and why the successive maxima are so variable in amount. This cause may, perhaps, be found in part in the position of Jupiter and Saturn in their orbits at the time of conjunction and opposition; but to render the explanation complete we seem obliged to admit that the magnetic condition of the sun has undergone a decided change within the past century; for whether we compare the times of maximum and minimum of the solar spots with the heliocentric longitudes of Jupiter and Saturn, or with their conjunctions and oppositions, we find large differences in the apparent influence of these planets in successive revolutions, and these differences go on steadily increasing for several revolutions of Jupiter; in other words the action of Jupiter and Saturn does not appear to be always the same in the same part of their orbits, nor does the maximum of the spots always occur at the same interval from conjunction or opposition of the planets.

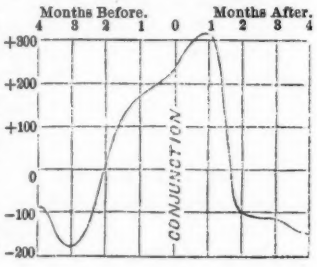
That the mean period of the solar spots is determined mainly by the conjunctions and oppositions of Jupiter and Saturn, is rendered probable by a similar action of Venus and the earth. The observations of Mr. Carrington from 1854 to 1860, have been carefully reduced by Messrs. De La Rue and Stewart (Researches on Solar Physics, second series, 1866) so as to show the actual area of the sun's spotted surface for each day of observation for seven years. In order to eliminate the effect produced upon the amount of spotted surface by the ten yearly period, Messrs. D. and S. have taken the difference between the

actual amount of spotted surface for each month, and the amount indicated by the normal ten yearly curve. These differences are mainly the effect of causes whose period is less than 10 years. If now we arrange these numbers in appropriate columns regulated by the times of conjunction of Venus and the earth, and take the averages of the different columns, we shall find that the results follow (in the main) an obvious law, as will be seen in the following table, showing the

Spotted surface of the sun dependent upon the conjunctions of Venus and the Earth.

	Months Before.				Con-	Months After.			
	4	3	2	1	junc.	1	2	3	4
1854 Feb. 28. Inf. conj.									
" Dec. 12. Sup. conj.				−198	− 34	− 47	+ 10	−147	−107
1855 Sept. 30. Inf. conj.	− 88	+ 5	− 73	+311	− 75	+369	+114	− 50	− 93
1856 July 20. Sup. conj.	− 63	− 48	− 50	− 38	− 35	+ 35	− 2	+ 40	0
1857 May 9. Inf. conj.	0	+ 15	0	+ 9	+ 20	+138	− 30	− 48	− 55
1858 Feb. 28. Sup. conj.	− 5	− 30	−165	+ 16	+240	− 83	+ 20	−160	+280
" Dec. 13. Inf. conj.	+ 45	−177	− 90	+419	+ 62	+494	−364	−214	− 58
1859 Sep. 27. Sup. conj.	−167	+183	+443	+488	+285	+960	−235	− 65	−670
1860 July 18. Inf. conj.	−115	−460	+625	+640	+900	+750	+135	−300	−670
Mean	−305	−900	−500	−180	+720	+235	−565	− 25	+100
	− 87	−177	+ 24	+163	+231	+317	−102	−108	−141

The curve of the accompanying figure represents these average results, and shows that the principal part of these differences may be represented by a simple curve whose period is 9·7 months; being the period between the successive conjunctions of Venus and the sun, or the heliocentric conjunctions and oppositions of Venus and the earth.



The preceding facts are thought to warrant the following propositions:

1. The mean interval between the successive maxima of the solar spots is almost exactly ten years.

2. The value of the maximum of the solar spots is variable, there being generally two or three successive maxima of unusual magnitude, followed by two or three maxima which are smaller than the average magnitude.

3. The ten yearly period of the solar spots is determined by the heliocentric conjunctions and oppositions of Jupiter and Saturn.

4. The principal fluctuations in the amount of the sun's spotted surface from the normal ten-yearly curve, are determined by the heliocentric conjunctions and oppositions of Venus and the earth.

5. The fluctuations in the period of the solar spots from 7 to 13 years, and the great fluctuations in the amount of the suc-

cessive maxima cannot be explained simply by the configuration of the planets, without also admitting a secular change in the magnetism of the sun analogous to the secular change which has been observed in the magnetism of the earth.

Diurnal inequality of the magnetic Declination.—In comparing the diurnal inequality of the magnetic Declination with the amount of spotted surface of the sun, I have taken the observations at Prague as the standard, because these observations are at least as suitable for this purpose as those of any other observatory, and because they are the observations which Professor Wolf has chiefly employed in his comparisons. During the eighteenth century, observations of the magnetic declination were made at Montmorency, France; at Mannheim, Baden; at Paris and London. Those of Montmorency extend from 1777 to '79, and were published in the Connaissance des temps for 1780, etc., and are copied in the Zürich Vierteljahrsschrift, v. 5, p. 241; those of Mannheim extend from 1781 to 1790, and were published in the Palatine Ephemerides, and are copied in the Vierteljahrsschrift, v. 6, p. 427; those of Paris were made by Cassini, and extend from 1784 to '88, and are copied in the Vierteljahrsschrift, v. 2, p. 291; and those of London extend from 1786 to 1805, were made by Gilpin, and were published in the London Philosophical Transactions for 1806, p. 416. During the present century, before the commencement of the Prague observations, we have Beaufoy's observations at London from 1813 to '20; Arago's observations at Paris from 1821 to '31; and the Göttingen observations from 1834 to 1840.

In order that all these observations may be properly represented by a continuous curve, a correction must be applied, not only for difference of locality, but also for a difference in the hours of observation, and for a difference in the mode of deducing from the observations the mean diurnal change. At Prague, the mean diurnal change for the year is obtained by taking the difference between the mean declinations at 8 A. M. and 2 P. M. But the greatest declination usually occurs at 1 P. M., so that the diurnal change appears greatest at those observatories which select the hours of 8 A. M. and 1 P. M. Also the critical hours vary somewhat with the season of the year, so that we shall obtain a still greater value of the diurnal change if we combine the hours of maximum and minimum for each month separately. Moreover, at Greenwich it is customary, in making these determinations, to reject those observations which were made in times of great magnetic disturbance. If such observations are retained, their effect will be to increase considerably the amount of the mean diurnal change. It is difficult to decide what correction should be applied to the different series of observations above enumerated, in order to reduce

them to the Prague standard. Fortunately, however, this correction scarcely at all affects the times of maximum and minimum, but simply removes what would otherwise appear as anomalies in the amount of the diurnal fluctuation for different years. The corrections which I have actually applied are the following:

The observations at Montmorency have been increased by one-fifteenth part.
" " " Mannheim diminished by one-fifteenth.
" " " Paris, 1784–88, diminished by one-fifth.
" " " London, 1786–1820, diminished by one-tenth.
" " " Paris, 1821–31, diminished by one-fourth.
" " " Göttingen, 1834–40, diminished by one-fifteenth.

The following table presents a summary of all these observations: [See page 161.]

These observations, after being reduced to the Prague standard in the manner already stated, are represented by the middle curve traced on the accompanying Plate. This curve bears an obvious resemblance to the curve of solar spots, and in general may be said to be parallel with it. The differences between the two curves prior to 1834, may be suspected to arise in part from errors in the observations of one or both of the phenomena represented. Since 1834 the general correspondence of the two curves is remarkably close, but real differences are indicated in the years 1836–8, in 1842–4, and in 1864.

This comparison seems to warrant the following propositions:

1. A diurnal inequality of the magnetic declination, amounting at Prague to about six minutes, is independent of the changes in the sun's surface from year to year.

2. The excess of the diurnal inequality above six minutes as observed at Prague, is almost exactly proportional to the amount of spotted surface upon the sun, and may therefore be inferred to be produced by this disturbance of the sun's surface, or both disturbances may be ascribed to a common cause.

Periodical recurrence of great auroral displays.—In attempting to decide whether auroral displays exhibit the character of a true periodicity, a careful discrimination is necessary in selecting our data for comparison. Travelers who have wintered in some parts of the Arctic regions, have reported that auroral displays were witnessed almost every clear night when the light of the sun did not interfere, even on those years when in the middle latitudes auroras were least frequent. If then we construct a complete catalogue of all recorded auroras, including the 142 auroras observed at Fort Enterprise in 1820–1, the 143 auroras observed at Bossekop in 1838–9; the 141 auroras observed at

Moose Factory in 1850–1; and the 131 auroras observed at Point Barrow in 1852–3, etc., we introduce into our series of numbers an anomaly which results simply from a change of the station of observation, and our numbers do not represent the relative frequency of auroras on different years for any single station or region of the earth. All observations then from the polar regions which are limited to occasional years, and do not form a continuous series, should be entirely discarded in the present comparison. We desire to know

Diurnal inequality of the Magnetic Declination.

Year.	Montmorency.	Mannheim.	Paris.	London.	Reduc'd to Prague.	Year.	Paris.	Gottingen.	Reduced to Prague.	Prague.
1777	11′·2	------	------	------	12′·0	1828	11′·52	------	8′·64	------
1778	10·0	------	------	------	10·7	1829	13·74	------	10·31	------
1779	8·5	------	------	------	9·1	1830	12·40	------	9·30	------
1781	------	9′·12	------	------	8·51	1831	11·75	------	8·81	------
1782	------	8·11	------	------	7·57	1832	------	------	------	------
1783	------	8·77	------	------	8·19	1833	------	------	------	------
1784	------	6·98	9′·65	------	7·12	1834	------	8′·47	7·91	------
1785	------	8·56	10·80	------	8·32	1835	------	9·57	8·93	------
1786	------	12·01	14·00	14′·40	11·79	1836	------	12·34	11·52	------
1787	------	------	15·14	14·98	12·80	1837	------	12.27	11·45	------
1788	------	------	13·48	13·81	11·61	1838	------	12·74	11·89	------
1789	------	8·75	------	11·87	9·43	1839	------	11·03	10·29	------
1790	------	8·33	------	11·95	9·27	1840	------	9·91	9·25	8′·84
1791	------	------	------	11·42	10·28	1841	------	------	------	7·43
1792	------	------	------	9·12	8·21	1842	------	------	------	6·34
1793	------	------	------	8·43	7·59	1843	------	------	------	6·57
1794	------	------	------	7·25	6·53	1844	------	------	------	6·05
1795	------	------	------	6·94	6·25	1845	------	------	------	6·99
1796	------	------	------	7·20	6·48	1846	------	------	------	7·65
1797	------	------	------	7·52	6·77	1847	------	------	------	8·78
1798	------	------	------	7·28	6·55	1848	------	------	------	10·75
1799	------	------	------	7·16	6·44	1849	------	------	------	10·27
1800	------	------	------	6·74	6·07	1850	------	------	------	9·97
1801	------	------	------	7·68	6·91	1851	------	------	------	8·32
1802	------	------	------	8·22	7·40	1852	------	------	------	8·09
1803	------	------	------	9·18	8·26	1853	------	------	------	7·09
1804	------	------	------	8·12	7·31	1854	------	------	------	6·81
1805	------	------	------	8·16	7·34	1855	------	------	------	6 41
1813	------	------	------	6·83	6·15	1856	------	------	------	5·98
1814	------	------	------	7·62	6·86	1857	------	------	------	6·95
1815	------	------	------	7·66	6·89	1858	------	------	------	7 40
1817	------	------	------	8·55	7·70	1859	------	------	------	10·44
1818	------	------	------	8·81	7·93	1860	------	------	------	10·05
1819	------	------	------	7·77	6·99	1861	------	------	------	8·46
1820	------	------	------	7·79	7·01	1862	------	------	------	7·48
1821	------	------	9·10	------	6·82	1863	------	------	------	7·33
1822	------	------	8·83	------	6·62	1864	------	------	------	6·72
1823	------	------	8·18	------	6·14	1865	------	------	------	------
1824	------	------	8·20	------	6·15	1866	------	------	------	------
1825	------	------	9·67	------	7·25	1867	------	------	------	6·47
1826	------	------	9·76	------	7·32	1868	------	------	------	7·27
1827	------	------	11 31	------	8·48	1869	------	------	------	------

whether auroral displays exhibit a periodicity in any given locality or region, and for this purpose we desire a long series of faithful observations at a single station or a limited number of stations; or if satisfactory observations of this kind cannot be obtained, then we must employ observations from a limited region of country where the records have been the most complete and continuous.

The observations at New Haven and Boston combined (Smith. Report, 1865, p. 225,) form a tolerably complete series from 1742 to 1854, and these numbers correspond in a remarkable manner with the fluctuations in the solar spots; but since during a considerable portion of this period no systematic watch for auroras was maintained, it is desirable to combine with this series, observations made at other localities. The observations at St. Petersburg (Smith. Rep. 1865, p. 227) are continuous from 1726 to 1811; and the observations at Berlin (Heis Wochenschrift, Jan. 1870, p. 47) are continuous from 1700 to 1800. I have combined these observations with those at New Haven and Boston, and the correspondence of the resulting numbers with the fluctuations of the solar spots is quite satisfactory. Inasmuch, however, as I have not been able to obtain a continuation of the Petersburg and Berlin observations to the present time, I have thought it best to extend the area of observation, and include all published observations from Europe south of the parallel of 55°. By this method of comparison we eliminate to a considerable extent the anomalies introduced into the observations of any single station, by cloudy weather which conceals many auroras from view; and we also partially eliminate the anomalies resulting from the unequal diligence of the many different observers who must necessarily participate in a series of observations extending over more than a century.

In the Vierteljahrsschrift, vol. 10, p. 232, is given a very complete catalogue of European auroras classified by parallels of latitude, and I have used this as my principal basis in the subsequent comparisons. In the table on page 163, column first shows the year of observation; column second the number of auroras recorded in Europe south of the parallel of 55°; column third the auroras at New Haven and Boston; column fourth the sum of the numbers in the two preceding columns. In order to eliminate still further the effects of irregular and non-periodic causes, I have taken the averages of the numbers in column fourth for each successive period of three years, and the numbers thus resulting are given in column fifth.

For a few years preceding and following 1787, column second shows a very remarkable increase in the number of observed auroras sufficient to excite a suspicion that these numbers are relatively too great. The Palatine Meteorological Society, which

Number of Auroras observed on different years.

Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.	Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.	Year.	Europe south of 55°.	Boston and New Haven.	Sum of do.	Mean for three years.
1739	46		46		1783	73	22	58	47	1827	16	7	23	17
1740	23		23	39	1784	51	4	29	39	1828	12	6	18	21
1741	49		49	39	1785	42	9	30	56	1829	20	2	22	25
1742	42	2	44	43	1786	110	55	110	84	1830	28	6	34	25
1743	33	2	35	31	1787	133	47	113	108	1831	17	2	19	20
1744	14	0	14	20	1788	124	38	100	105	1832	6	2	8	13
1745	10	0	10	16	1789	103	51	102	84	1833	8	3	11	11
1746	17	7	24	22	1790	77	13	51	68	1834	4	9	13	12
1747	23	10	33	27	1791	78	12	51	46	1835	7	6	13	15
1748	18	6	24	27	1792	60	6	36	37	1836	14	5	19	32
1749	13	10	23	33	1793	15	8	23	23	1837	22	41	63	42
1750	34	17	51	31	1794	8	2	10	14	1838	5	39	44	60
1751	14	5	19	30	1795	6	2	8	7	1839	27	47	74	65
1752	19	2	21	17	1796	2	0	2	8	1840	32	44	76	80
1753	11	1	12	15	1797	13	0	13	5	1841	34	56	90	67
1754	12	0	12	11	1798	1	0	1	6	1842	18	18	36	53
1755	10	0	0	10	1799	5	0	5	4	1843	24	10	34	37
1756	7	0	7	9	1800	6	0	6	6	1844	28	13	41	39
1757	4	6	10	7	1801	8	0	8	6	1845	18	24	42	48
1758	1	4	5	14	1802	2	2	4	6	1846	25	35	60	54
1759	22	5	27	16	1803	1	5	6	6	1847	35	25	60	76
1760	11	6	17	23	1804	4	4	8	14	1848	46	62	108	72
1761	20	5	25	22	1805	23	4	27	14	1849	26	23	49	69
1762	16	7	23	19	1806	3	4	7	13	1850	15	36	51	49
1763	2	6	8	16	1807	2	2	4	4	1851	20	26	46	65
1764	4	12	16	11	1808	1	0	1	2	1852	33	64	97	67
1765	1	7	8	8	1809	0	2	2	1	1853	20	39	59	63
1766	0	0	0	5	1810	1	0	1	1	1854	7	27	34	35
1767	4	4	8	9	1811	0	0	0	0	1855	0	12	12	20
1768	12	7	19	30	1812	0	0	0	1	1856	5	8	13	11
1769	46	18	64	40	1813	2	0	2	4	1857	1	7	8	21
1770	22	14	36	41	1814	6	3	9	5	1858	11	30	41	37
1771	7	15	22	24	1815	2	1	3	5	1859	47	16	63	50
1772	6	7	13	26	1816	2	0	2	5	1860	28	17	45	48
1773	26	17	43	33	1817	11	0	11	7	1861	24	12	36	36
1774	24	20	44	33	1818	4	4	8	11	1862	23	5	28	28
1775	7	5	12	22	1819	9	6	15	10	1863	19	2	21	29
1776	5	4	9	26	1820	4	2	6	8	1864	33	6	39	33
1777	41	15	56	38	1821	2	0	2	4	1865	36	2	38	34
1778	30	18	48	69	1822	2	1	3	2	1866	21	5	26	31
1779	98	4	102	70	1823	0	0	0	1	1867	9	20	29	22
1780	34	25	59	67	1824	0	0	0	1	1868	3	7	10	32
1781	68	25	59	57	1825	2	2	4	4	1869	28	28	56	
1782	58	24	53	57	1826	9	0	9	12	1870				

embraced a large number of zealous observers, was organized in 1780 and continued to publish an annual volume of observations till 1792. During this period, auroras were made a special subject for observation, and it cannot be doubted that they were recorded with more fidelity than for most of the preceding and following years. I have therefore reduced the European ob-

servations during this interval by one-half, and have combined this result with the New England observations in completing the numbers in the last two columns.

The Boston observations published by Prof. Lovering in the Memoirs of the American Academy, vol. ix, p. 101, and copied in the Smithsonian Report for 1865, p. 227, extend only to 1848. In order to obtain a continuation of this series to the present time, I have carefully consulted the Meteorological Journal kept at Cambridge (Mass.) Observatory, and have obtained the following results :

Number of Auroras recorded each year at the Cambridge Observatory.

Year.	No.	Year.	No.	Year.	No	Year.	No.	Year.	No.
1841	27	1847	9	1853	27	1859	16	1865	1
1842	11	1848	17	1854	25	1860	12	1866	0
1843	4	1849	4	1855	12	1861	6	1867	20
1844	6	1850	15	1856	8	1862	0	1868	6
1845	7	1851	17	1857	7	1863	1	1869	28
1846	15	1852	44	1858	30	1864	1		

When auroras were recorded at New Haven which were not recorded at Cambridge, I have added them to the preceding numbers, and the result is given in column third of the table on page 163. In the years 1860, 1 and 2, no record of auroras was preserved at New Haven, and to supply this omission I have inserted for these years the observations made at Middletown, Conn., by Prof. John Johnston. The European observations published in the Vierteljahrsschrift close with 1861. I have continued the table to the present time by means of the observations published each year in Heis Wochenschrift, confining myself to such observations as were reported south of lat. 55°.

The numbers given in column fifth have been projected in a curve represented at the top of the accompanying Plate. This curve shows an unmistakable resemblance to the curve of the solar spots, and to that of the magnetic declination. To each maximum of the two latter curves there corresponds a maximum of the auroral curve, not always simultaneously, but not differing from it more than a single year except in one or two instances. The following table shows the departures of the auroral maxima from the maxima of the solar spots.

Date.	Diff.	Date.	Diff.	Date.	Diff.
1750	−1 year.	1787	0 year.	1837	+3 years.
1761	0 "	1804	0 "	1848	−1 "
1769	+1 "	1816·5	+1·5 "	1860	−1 "
1778	0	1830	−1 "		

The correspondence in the times of minimum is still more remarkable as is shown in the following table.

Date.	Diff.	Date.	Diff.	Date.	Diff.
1744	+1 year.	1784	0	1838	0
1755·5	+1·5 "	1798·5	unc'n.	1843	0
1766	0 "	1810	0	1856	0
1775	0 "	1823	0	1867	0

There are only two instances in which there is any discrepancy in the times of minima. In one of them (1744) the observations of the solar spots are confessedly very uncertain, and in the other (1755) the change in the number of observed auroras from 1755 to 1757 was only *three*, so that we seem authorized to assert that there is an invariable coincidence in the times of minima of the solar spots and of auroral displays.

The range of the maxima and minima of auroral displays is considerably greater than that of the solar spots, and the observations of magnetic declination seem to indicate a similar peculiarity. There seems then to be no room for doubt that auroral displays exhibit the ten yearly period of solar spots, but the range of the changes on different years is subject to influences which may be independent of the sun.

In order to decide whether this periodicity is exhibited in the higher latitudes, I have selected all those observations with which I am acquainted which show more than a hundred auroras in a year. The following is the table.

Date.	Place.	Lat.	Long.	Numb. Auroras.	Authority.
1820–1	Cumberland House,	53° 56′	102° 16′	142	Force (Smith. Cont.), p. 6.
1820–1	Fort Enterprise,	64 28	113 6	142	Force (Smith. Cont.), p. 24–35.
1833–5	Great Slave Lake.	62 46	109 1	105	Capt. Back.
1838–9	Bossekop,	69 58	23 34E	143	Voyage en Scandinavie.
1848–9	Fort Confidence,	66 54	118 49	122	Athabasca obs. p. 324–350.
1850–1	Moose Factory,	51 10	81 0	141	Am. Jour. Sci., [2], xiv, 156.
1850–1	Athabasca Lake,	58 43	111 18	109	Am. Jour. Sci., [2], xiv, 156.
1852–3	Point Barrow,	71 21	156 15	131	Phil. Trans. 1857, p. 497.

These places are all situated within the zone of greatest auroral frequency, Am. Jour., vol. xxx, p. 89. The observations of 1820–1 and 1833–5 were made near the time when the disturbance of the sun's surface was a minimum; while those of 1848–9 were made when the disturbance of the sun's surface was a maximum; and those of 1838–9, and 1850–1 were made when the disturbance was greater than the average. The average number of auroras recorded at the first two dates is nearly the same as at the last three dates. So far as any conclusion is warranted from so limited a number of observations, the inference is that within the zone of greatest auroral frequency, the number of auroras is about the same for all years, independent of the disturbed condition of the sun's surface. It is presumed that there must be a periodical change in the *brilliancy* of the auroral displays, but the observations are not sufficiently numerous and precise to justify us in pronouncing positively upon this point. As we recede from the zone of greatest auroral frequency, the influence of the sun is shown in a periodical change in the number of auroras; and this periodicity in Europe appears to be the most distinctly marked near the parallel of 50° latitude; and in New England near the parallel of 42° latitude.

It will be observed that the change from a maximum to a minimum of the solar spots is not uniform, and frequently the inequality is so marked as to assume the form of a distinct secondary maximum. Such maxima occurred in 1774, 1792, 1794 (if this is not to be regarded as a primary maximum) and 1864; and in several other cases the inequality in the progress from a maximum to the succeeding minimum is decided. The mean daily range of the magnetic declination exhibits also secondary maxima, and generally the small inequalities in the curve of magnetic declination bear some correspondence to the small inequalities in the curve of the solar spots. The auroral curve shows still greater irregularities, and it is remarkable that an irregularity in the auroral curve often has a decided corresponding irregularity in one of the other two curves. Thus both the auroral and sun-spot curves show a decided maximum in 1774; also in passing from the maximum of 1778 to the following minimum the undulations of the three curves bear a marked resemblance; in the descent from the maximum of 1787, there is a secondary maximum of the magnetic curve corresponding to that of the solar-spot curve; in the descent from the maximum of 1848 there are corresponding undulations of the sun-spot curve and the magnetic curve, while the auroral curve shows at the same time a well-marked secondary maximum; and nearly the same remark is applicable to the descent from the principal maximum of 1860.

The following conclusions are thought to be warranted by the preceding observations.

1. Within the zone of greatest auroral frequency, auroras are of almost daily occurrence in all years, and it is doubtful whether in this region there is any decided periodicity in the number of auroral displays, although there may be periodical changes of brilliancy.

2. At places where the average number of auroras is about 20 or 25 annually, the ten-yearly period of the solar spots can be distinctly traced.

3. The times of maximum and minimum of the solar spots correspond in a remarkable manner with the times of maximum and minimum in the frequency of auroral displays in the middle latitudes.

4. The successive maxima of auroral displays are more variable than those of the solar spots, so that the ten-yearly period might be easily overlooked, and it might be inferred that the maxima only occurred at intervals of about 60 years.

5. The most remarkable irregularities in the auroral curve in the progress from a maximum to a minimum, correspond to similar though generally smaller inequalities in the curve of solar spots or of magnetic declination.

Magnetic storms compared with the prevalence of solar spots.

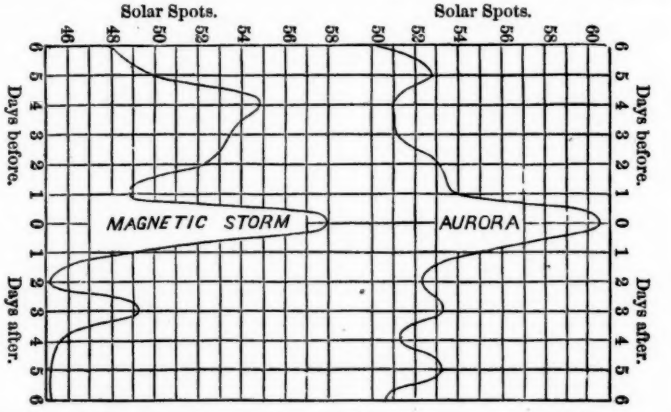
Since the mean daily range of the magnetic needle is greatest on those *years* in which the sun's surface is most disturbed, we could not be surprised if the range of the magnetic needle should prove to be the greatest on those *days* on which the solar spots are most extensive. In order to test this question, I have compared the extent of the solar spots for 6 days preceding and 6 days following each of the great magnetic disturbances at Greenwich for a period of 23 years. In the Greenwich observations for 1862 is given an abstract of the magnetic observations on 177 days of great magnetic disturbance from 1841 to 1857; and in the observations for 1867 are enumerated the days of great magnetic disturbance from 1858 to 1863, amounting in number to 45. These 23 years furnish 222 days of great disturbance, and I have compared Wolf's numbers representing the extent of the solar spots near the time of these dates. Having prepared a table with 13 vertical columns, I insert in the middle column, Wolf's relative number for one of the days of great magnetic disturbance, and I also insert in their appropriate columns the relative numbers for the 6 preceding and 6 following days. In this manner I have treated all the days of great magnetic disturbance at Greenwich, with the exception of those cases in which very few observations of the solar spots were made, and a few cases in which there were two or three successive days of great magnetic disturbance, in which cases I have generally selected the day of greatest disturbance and made but one entry of that period in the table. The cases of disturbance which I have thus employed amount to 135. The following table (see page 168) exhibits these cases for ten years from 1848 to 1857, and at the bottom of the table are given the averages of these numbers for each of the 13 columns; and in another line are given the corresponding averages for the whole number of 135 cases of magnetic disturbance.

The details for the remaining 74 days of observation are omitted in order to avoid incumbering the pages of this Journal. The resulting averages for the 135 cases are represented by the lower curve line in the figure, page 169, where the middle vertical line represents the day of a magnetic storm at Greenwich, and the other vertical lines represent 1, 2, 3, etc., days *before* the storm, and also 1, 2, 3, etc., days *after* the storm. The horizontal lines indicate the amount of disturbance of the sun's surface as measured by Wolf's relative numbers extending from 45 to 58. From this curve it will be seen that the disturbances of the sun's surface about the time of a magnetic storm, bear an analogy to the waves in our own atmosphere about the period of a violent winter storm. We find a well-

Spotted surface of the sun near the time of a magnetic storm.

		Days before the storm.						Mag-netic Storm	Days after the storm.					
		6	5	4	3	2	1		1	2	3	4	5	6
1848.	Jan. 28,			100		133	147	151	135	127	105	90		107
	Feb. 21,	116	90		70		20	103	50	106		60	50	
	March 17,	80	106	76		101	60	113	101	75	95	93	79	78
	May 18,	50	91	96	90	54	113	99			85	72	88	68
	July 11,	93	105	112	160	99	122	170	132	126	70	50	77	60
	Oct. 23,		150			70	70	142	94	60	90	40	74	88
	Nov. 17,	20	79		136	163	156		80	60	60		70	54
	Dec. 17,	129	141	133	133	100		105	142	100	135	125	90	170
1849.	Oct. 30,	58	44			34	61	118	66	66	111	40	71	
1850.	Feb. 22,	58	73	80	122	154	55	115	54	60	56	78	75	69
	March 31,	57	23	59	39	67	37	39	27	27	37	0	24	0
	May 7,	25	86	14	36	60	22	80	46	50	74	101	56	92
	June 13,	46	15	132	133	147	129	67	51	34	60	48	56	80
	Oct. 2,		25		45	45	35	55	40	34	39	59	40	22
1851.	Jan. 19,	58	87	27	57	20	20	62	45	23		37	13	11
	Feb. 18,	79	78	46	108	73	105	78	101	105	47	122	116	86
	Sept. 4,	35	46	38	102	40	24	103	66	35	45	69	70	52
	Oct. 28,	101	94	63		82	36	50	54	40	20	37	48	22
	Dec. 6,	79	87	81	46	47	38	26	13	38	36	53	41	
	Dec. 28,	76	60	30	10	71	23	96	94	47	117	82	82	59
1852.	Jan. 4,	94	47	117	82	82	59	52	70	61	83	6	50	66
	Jan. 19,	6	7	60	40	6	96	127	106			14	74	76
	Feb. 15,	2	48			77	77		144	62	16	15	101	10
	Feb. 20,	77		144	62	16	15	101	10	5	77	54	46	15
	April 20,	62	64	34	53	51	38	47	49	28	16	13	50	50
	May 20,	38	46	36	37	11	11	29	28	31	42	57	64	59
	June 11,	57	66	56	44	1	0	11	30	28	4	4	30	24
	June 16,	0	11	30	28	4	4	30	24	25	22	33	48	26
	July 10,	51	65	65	70	11	56	61	34	24	0		25	0
	Nov. 11,	48	10	105	14	81	81	73	61	11	47	34	11	30
1853.	Jan. 10,	25	39	29	27		11	11	23	12	12	11	24	26
	March 7,	15	17	15	12	0	0	11		14	11	11	22	23
	May 3,	26	0	11	14	41	27	31	26	21	16	14	22	13
	May 24,	30	35	47	48	71	46	20	49	59	54	23	29	10
	June 22,	65	26	67	56	56	51	11	20	11	11	11	11	11
	July 12,	35	45	48	75	69	47	48	71	35	63	79	49	47
	Sept. 1,	53	67	33	33	37	56	57	44	68	55	23	34	
	Oct. 25,	48	49	50	46	30	44	44	40	32	26	36	25	
	Nov. 9,			25	25	25		22	22	34		35	25	23
	Dec. 6,	13	11	12	17	17	14	17	17			29	38	18
	Dec. 21,		32	20	31		22	11			11	14	13	
1854.	Jan. 8,	19	27	10	17	10	25	25	0	17	19	19	17	13
	Jan. 20,	13	14	15	17	17	16	14	11	0	0	0	0	0
	Feb. 16,	26	29	35	28	17	10	22	25	17	17	11	0	0
	Feb. 24,	17	17	11	0	0		12	0	0	0	0	12	12
	March 6,	0	12	12	0	0	0	0	0	0	11	34	45	44
	March 15,	11	34	45	44	39	33	33	23	26	27	33	33	27
	March 28,	25	11	13	11	29	22	35	11	0	11	12	12	36
	April 10,	37	38	49	49	56	54	54	55	55	36	35	14	12
	April 23,	12	13	24	22	25	12		11	11	0	0	0	0
	May 25,	13	11	11	13	13	11	0	0	0	0	0	0	14
1855.	March 12,	29	12	38	23	11	16	12	11	14	11	0	11	0
	April 4,	12	16	17	17	12	0	11			13	0		0
	Oct. 18,	15	12	0	0	0	0	0	12	17	11	12	13	11
1857.	March 13,	0	0	0	11	13	12	0	11	11	13	11	13	0
	May 7,	17	16	16	15	15	25	24	20	25	25	26	22	20
	May 10,	15	15	25	24	20	25	25	26	22	20	42	53	60
	Sept. 3,	39	27	27	26	37	38	35	27	48	50	40	29	58
	Nov. 12,	32	26			24	24	24	24	24	36	50	38	58
	Nov. 16,	24	24	24	24	24	36	50	38	58	50	23	45	46
	Dec. 16,	11					71	79	68	54	68			42
Mean of 61 days obs.		40·6	44·4	46·5	46·1	45·8	42·4	52·4	46·2	38·8	40·0	36·6	40·8	38·2
Mean of 135 d'ys obs.		47·9	50·1	54·8	53·5	52·3	48·9	57·9	49·0	45·1	49·3	45·6	45·2	45·3

marked maximum on the day of the magnetic storm; a second maximum four days before the storm, which is also pretty dis-



tinctly marked; and a third maximum, more uncertain, three days after the storm. The entire fluctuation within a period of two days is from 45·1 to 57·9, or 28 per cent of the smaller quantity, an effect which is so large and derived from a comparison of so many cases as to indicate a law of nature. Hence we seem authorized to conclude

1. Great disturbances of the earth's magnetism are accompanied by unusual disturbances of the sun's surface on the very day of the magnetic storm; and are, therefore, due to some influence which emanates immediately from the sun.

2. The great disturbance of the sun's surface which accompanies a terrestrial magnetic storm, is generally heralded by a smaller disturbance three or four days previous, succeeded by a comparative calm which immediately precedes the magnetic storm.

Auroral displays and Solar spots compared.

Since the average disturbance of the sun's surface is greatest on those *years* in which auroras are most prevalent, we could not be surprised if the disturbance of the sun's surface should prove to be the greatest on those *days* on which auroras prevail in the middle latitudes. In order to test this question, I have proceeded in the same manner as in the case of magnetic storms; and in order to avoid the suspicion of having selected a few cases for the purpose of confirming a preconceived theory, I have taken the entire series of observations made by Messrs. Herrick and Bradley at New Haven, from 1837 to 1854. These observations are 514 in number, and I have employed them all with the following exceptions:

Spotted surface of the Sun near the time of an Auroral display.

		Days before the Aurora.							Days after the Aurora.					
		6	5	4	3	2	1	Aurora	1	2	3	4	5	6
1850.	Jan. 18,	79	62	73	27	74	65	20	20	15	33		37	56
	Feb. 3,	15	14	79	54	87	75	113	86	86	22	20	30	102
	Feb. 22,	58	73	80	122	154	55	115	54	60	56	78	75	69
	March 3,	56	78	75	69	93	71	97	45	94	56	65	51	168
	March 10,	45	94	56	65	51	168	98	89	65	69	78	70	64
	March 31,	57	23	59	39	67	37	39	27	27	37	0	24	0
	April 7,	27	27	37	0	24	0	12	27	11		30	31	15
	May 7,	25	86	14	36	60	22	80	46	50	74	101	56	92
	May 12,	22	80	46	50	74	101	56	92	50	11	52	34	33
	July 12,	0	12	11	24	46	12	76	33	110	105	110	77	40
	Aug. 9,	77	71	35	47	42	65	60	13	59	47	13	12	51
	Sept. 4,	54	13	82	81	126	105	107	86	104	106	98	86	106
	Sept. 10,	107	86	104	106	98	86	106	74	118	78	58	76	52
	Sept. 30,	46	50		25		45	45	35	55	40	34	39	59
	Oct. 2,		25		45	45	35	55	40	34	39	59	40	22
	Oct. 27,	46	62	40	40		58		86	15	27	68	14	20
	Dec. 26,	81	66	71	60	40	27	58	48	35	39			55
1851.	Feb. 18,	79	78	46	108	73	105	78	101	105	47	122	116	86
	March 22,	31	46	23	47	57	64	81	62	38	54	40	27	35
	March 29,	62	38	54	40	27	35	35	53	54	28	45	12	34
	June 5,	36	81	95	74	27	49	51	44	37	31	42	37	39
	June 27,	108	96	40	34	49	64	61	41	71	41	35	11	12
	July 17,	45	36	34	34	66	45	30	33	52	55	54	35	54
	Sept. 4,	35	46	38	102	40	24	103	66	35	45	69	70	52
	Sept. 7,	102	40	24	103	66	35	45	69	70	52	76		79
	Sept. 10,	103	66	35	45	69	70	52	76		79	80	45	60
	Oct. 1,	40	38	30				14	14	47	109	14	43	32
	Oct. 20,	48	51	15	20	51	64	54	136	101	94	63		82
	Oct. 23,	20	51	64	54	136	101	94	63		82	36	50	54
	Oct. 28,	101	94	63		82	36	50	54	40	20	37	48	22
1852.	Jan. 19,	6	7	60	40	6	96	127	106			14	74	76
	Feb. 16,	48			77	77		144	62	16	15	101	10	5
	Feb. 20,	77		144	62	16	15	101	10	5	77	54	46	15
	March 7,	5	36	2	44	1	39	40	73	56	67	79	81	86
	April 10,	61	77	94	113	117	113	114	90	79	77	62	64	34
	April 15,	113	114	90	79	77	62	64	34	53	51	38	47	49
	April 22,	34	53	51	38	47	49	28	16	13	50	50	81	84
	May 17,	41	25	2	38	46	36	37	11	11	29	28	31	42
	June 5,	4	97	80		5	72	57	66	56	44	1	0	11
	June 11,	57	66	56	44	1	0	11	30	28	4	4	30	24
	June 16,	0	11	30	28	4	4	30	24	25	22	33	48	26
	July 6,	74	49	34	51	51	65	65	70	11	56	61	34	24
	July 10,	51	65	65	70	11	56	61	34	24	0		25	0
	Sept. 6,	49	37	47	34	35	38	35	61	4	38	2	29	38
	Sept. 17,	29	38	26	11	11	0	15	24		57	2	48	54
	Sept. 21,	11	0	15	24		57	2	48	54	44	41	2	33
	Sept. 29,	54	44	41	2	33	34	36	26	3	26	37	42	7
	Oct. 5,	36	26	3	26	37	42	7	37	26	3	27	28	42
	Oct. 20,	69		52	63	76	87	89	109	128	146	40	8	97
	Nov. 11,	48	10	105	11	81	81	73	61	11	47	34	11	30
	Dec. 5,	55	38		40	33	43	58	42	43	29	27	35	33
	Dec. 13,	43	29	27	35	33	27	28	26	14	28	24	10	15
1853.	Feb 14,			28	36	54	44		68	63	43	60	58	85
	March 14,		14	11	11	22	23	24	23		36	64	60	40
	March 27,	34	53	56	68	56	65	63	56	47	58	45	35	34
	April 6,	45	35	34	58	49	20	77	72	27	34	49	40	42
	May 2,	26	26	0	11	14	41	27	31	26	21	16	14	22
	June 1,	59	54	23	29	10	20	38	27	15	28	19	50	44
	June 9,	15	28	19	50	44	40	52	69	80	52	40	47	73
	July 12,	35	45	48	75	69	47	48	71	35	63	79	49	47
	Mean of 60 days obs.	48·9	49·3	47·4	49·5	52·5	52·3	56·3	53·2	47·1	48·6	47·5	41·8	47·6
	Mean of 251 days obs.	50·3	52·7	51·0	51·2	53·1	53·7	60·5	54·8	52·5	53·3	51·4	53·3	50·7

1. Those cases in which the observers were not entirely confident that there was any auroral display.
2. Those cases in which the corresponding observations of the solar spots were very incomplete; and
3. Those cases in which auroras were observed on two or three successive days, when I have generally selected the most remarkable aurora, and made but one entry for that period.

The number of auroras which I have thus discussed is 251. In order to furnish a specimen of these numbers, the table on page 170 is given, in which the arrangement is the same as in the table on page 168. At the bottom of the table are given the averages of these numbers for each of the 13 columns; and in another line are given the corresponding averages for the whole number of 251 auroras.

These final averages are represented by the upper curve line in the figure page 169, from which it will be seen that there is a well-marked maximum of solar disturbance corresponding to the date of an auroral display. The small fluctuations during the preceding and following days, bear some resemblance to the fluctuations attending magnetic storms, but they are so small in amount that no importance is attached to them. The entire fluctuation within 6 days extends from 50·3 to 60·5, or 20 per cent of the whole quantity; a number so large and derived from so many cases that it is thought to indicate a law of nature. Hence we conclude that

Auroral displays in the middle latitudes of America are generally accompanied by an unusual disturbance of the sun's surface on the very day of the aurora, and are, therefore, subject to some influence which emanates immediately from the sun.

These conclusions may be modified by a comparison of a longer series of observations, and especially by more accurate observations which furnish for each day an exact measurement of the extent of the sun's spotted surface. Such observations have been made for several years at the Kew observatory, and it is hoped that when published they will furnish the materials for the desired comparison. The observations for 1862 and 1863 have already appeared in the Philosophical Transactions for 1869, pp. 23–44; and it is expected that the observations for the subsequent years will soon follow.

ART. XVII.—*On a new Period in Chronology, called the Precession Period;* by J. W. FRENCH. (In a letter to the Editors.)

I PROPOSE in the place of the Julian Period in chronology, another which I will call the Precession-Period.

This latter will be found to have all the advantages of the other without its defects, and beyond these comparative utilities, to have chronological uses of its own which are great and various, both for the subdivisions of history, and for the vast cycles contemplated by science.

The Julian Period in chronology consists of 7,980 Julian years, that number being formed by the continual multiplication of 28, 19 and 15; that is to say, of the cycle of the sun, the cycle of the moon, and the cycle of indiction. The first year of the Christian Era is made the 4,714th of the Julian Period. By such an arrangement we can find for any year its golden number, its number for the solar cycle, and that for its Roman Indiction. Also, we have a fixed period reaching back in history among the local and broken cycles of different peoples and countries. These certainly are great advantages, and have secured for that Period acceptance and commendation.

But some of its defects are these. 1. It has an artificial element, that of the Roman Indiction, instead of having its foundation wholly in astronomy. 2. It is soon exhausted, and in the past, it does not reach far enough even for the Septuagint Chronology in history. 3. It furnishes no unit nor cycle for science.

The period which I propose is founded wholly on astronomy, is exhaustless by being recurrent, has its initial point sufficiently far back for any conceivable historical purposes, gives to science a worthy unit for the vast durations it contemplates, and with these inestimable advantages, has the two practical utilities of the Julian Period;—that of giving the elements needed in the almanac for every year, and that of extending into the past a long and unaltering standard for time.

I propose to take, as the chronological unit, the time for the precession of the equinoxes, 25,872 *years.* By a singular felicity, that number can be formed from the factors 28, 84, and 11. Now, 28 is the number of the solar cycle; 84 is a lunar cycle employed formerly by the Jews, and having peculiar uses in estimating long series of lunations. The other number, 11, is a lunar cycle employed by the early Christians, the errors of which almost exactly counteract those of the cycle of 84 years. These three multiplied together form 25,872. A subordinate felicity will at once be seen by a complete mathematician. *The number* 25,872 *can be divided and subdivided to the last, without a remainder, by a large number of divisors.*

Making the years Gregorian, and calling the initial year of the Christian Era, 0, *I place the beginning of the Precession Period at* 12,693 *B. C.*

Instantly we have by that arrangement, the advantages which have brought the Julian Period into favor.

We can find the solar cycle, the golden number, the Roman Indiction (correctly as we would from the Julian Period) by the following simple rules:

1. To express any year before or after Christ in the correspondent number in the Precession-Period: For any year before Christ, deduct from 12,693, the figures of the year B. C.: For any year after Christ, add to the same number (12,693) the figures of the year A. D.

Thus 752 B. C. (753 *historical reckoning*) is 11,941 P. P. The present year 1870 A. D. is 1,870+12,693=14,563 P. P. (We use P. P. as abreviation for Precession-Period.)

2. To find the Golden Number, Solar Cycle, and Roman Indiction for any year before or after Christ: 1. Turn the year by the first rule into the correspondent number of Precession Period: 2. Divide that number by 19 for the golden number, by 28 for the solar cycle, and by 15 for the Roman Indiction. In the three remainders you have the answer.

Thus the present year 1870 A. D., is 14,563 P. P. Dividing this latter number by 19, I find a remainder of 9; by 28, a remainder of 3; and by 15, a remainder of 13. Opening the Almanac for this year, I find the answer correct. It gives the Golden Number as 9, the Solar Cycle as 3, the Roman Indiction as 13.

The Precession-Period has then in this particular, equal utility with the Julian.

The other advantage, that of a fixed standard of time extending back in past history, it possesses, and adds the great benefit of adequate length in both directions, to 12,693 B. C., and on to 13,179 A. D.; the whole forming one precession. And this precession is not like the Julian Period, an arbitrary straight line stretched over a small portion of duration. It is a definite circle marked on the face of the heavens. The precession begins at 12,693 B. C. with the *point of the vernal equinox in the Zodiac near Spica Virginis*, a brilliant star, forming a good point of departure. When the whole circle of the Zodiac is swept, and the first degree of celestial longitude is again by *Spica Virginis*, the period is completed.

Surely it is better to adopt such a period for our almanacs and histories than the inferior one called the Julian.

But beyond the historical and chronological uses in the *subdivisions* of the Precession-Period, are the advantages of that period as a unit for Astronomy and Geology. We want a unit

larger than the year. A precession is a good one. Instead of cumbering a line of page with bewildering cyphers we can say 40, 100, or 1,000 precessions. Thus 40 precessions (and forty is a number easily remembered) would, by another felicity of this period, make a million of years, with a little over. We might call it a millionade, and give that again multiplications to form an *age*. An "age" might be 400 precessions.

Should we adopt any thing like this plan, we should have the same delightful surprise which we often experience, by finding that ancient races and nations have been along the same pathway which we imagined ourselves to be for the first time breaking and exploring. Six days, such as are His, who in His times as in His nature, must be what others are not; six immense periods are reckoned in the Creative work not only in Genesis, but in Oriental Records. Take 100 precessions. Divide them by that number, six. You have the very periods recognized by the Brahmins of India, each about 430,000 years.

West Point, June 7, 1870.

ART. XVIII.—*Upon the Atomic Volumes of Solid Compounds;* by FRANK WIGGLESWORTH CLARKE, S.B.

IN studying the atomic volumes of solid compounds, the materials at my command have been in some respects quite copious, and in others quite limited. Having been unable, through lack of opportunity, to make any new determinations of specific gravities, I have been forced to content myself with the data which are scattered through the various scientific publications. These, apart from the views expressed in nearly a hundred papers written by various chemists upon atomic volumes, consisted of about 1,900 determinations of the specific gravities of 912 different solids of definite constitution, exclusive of alloys. Much of this material had to some extent been already worked over, although the larger part of it had never been examined in this direction. In some cases there are data covering a whole series of compounds, in others only one or two members of a series have been studied, while again, for many important substances no determinations of specific gravity have ever been taken. Again, for some compounds there are many determinations by different authorities, and these bodies can be studied with much certainty; while for other compounds only single observations of specific gravity have been made, and these often with no pretence to rigid accuracy.

Here then at the very outset is a difficulty. If a substance has a very low specific gravity, and a very high atomic weight,

a minute error in the determination of the first value must, in calculating the atomic volume, become greatly multiplied; so that a single experimental observation only, may often lead to wholly erroneous results for the atomic volume. Thus, for instance, with Melene, $C_{30}H_{60}$, an error of only 0·04 in the specific gravity will alter the atomic volume about 24·00. And it is very common to find the determinations of specific gravity for a single compound differing more than 0·04. And, as I have already stated, many of the specific gravities observed have never had more than approximate accuracy claimed for them. Hence, one of the chief difficulties in comparing atomic volumes lies in deciding how great variations can be safely ascribed to experimental error. For this purpose, instead of directly comparing the actual atomic volumes with the results of theory, I have preferred to compare the specific gravities calculated from the latter with those really determined by experiment. Even here much care is needful, since errors are more likely to occur with some compounds than with others. Thus, there is much more danger of error in determining the specific gravity of anhydrous magnesic chlorid, than in taking that of baric sulphate.

Furthermore, many different modes of taking specific gravity are represented by the values from which I have to calculate. Some compounds have been examined in the form of powder, and others have been crystallized; some have been studied near their melting point, and others distant therefrom. Nevertheless, in spite of all these possibilities of error and chances of irregularity, certain curious series of relations between atomic volumes are easily demonstrated; which, taken together, hint strongly at a rather more general theory of atomic volumes than has hitherto been enunciated.

But, to begin with, we need a brief resumé of certain points which have been demonstrated by other observers.

First comes the axiomatic statement of Schröder that the atomic volume of a compound must equal the sum of the atomic volumes of its constituent parts. This at once suggests questions of interest. Although many compounds, especially certain sulphids, selenids, and tellurids, have atomic volumes equalling the sums of those of the *free* elements composing them, many other compounds have values lower than such sums, thereby indicating condensation. And some iodids have atomic volumes greater than the sums of those of the metal and the iodine. So here arises the question,—when a compound possesses a greater or less volume than the elements contained in it do in the free state, do those elements condense or expand in equal or in different ratios? It is really upon this point that the study of atomic volumes hinges; so that, stripped of all metaphysical notions of atoms and spheres of heat, the subject

simply concerns the distribution of the changes of volume undergone by the various elements in uniting to form compounds.

Second. It has been shown, most definitely by Schröder and Kopp, that, in many cases, when from the atomic volumes of certain series of salts, oxyds, &c., we subtract the atomic volumes of the respective metals contained in them, we obtain constant remainders which may be regarded as representing the values of the various radicals and of oxygen. Thus, if from the atomic volumes of one class of oxyds we remove the values of the metals, we obtain the remainder 2·6. With other oxyds we get 5·2, and with still others, 10·4. These are provisionally regarded as the atomic volumes of oxygen in its solid compounds. These different values for a single element are of course natural consequences of the fact that the various compounds containing it, undergo, in their formation, different degrees of condensation from the free elements composing them. But, in assuming these three numbers to be the atomic volumes of oxygen, one assumption is made which is by no means allowable, viz: that the metals entering into these compounds undergo no condensation themselves, all the change in volume taking place in the oxygen. But, some oxyds have atomic volumes lower than those of the metals contained in them. And yet the remarkable multiple relation between the three values quoted above cannot be due to accident. These points will be referred to again.

Third, we come to some results which I myself published rather more than a year ago.* In my last paper upon this subject I pointed out some curious multiple relations connecting the atomic volumes of similar solid elements. For the proofs of these relations I must refer to the above mentioned paper, but the values for the elements themselves I am obliged to cite here for further reference. I give the values for solids only. Li 11·4, Na 22·8, K 45·6, Rb 57·0. These stand to each other as 1 : 2 : 4 : 5. I 25·6, Tl 17·2, Ag and Au 10·2.

Here arises an interesting question. The atomic volumes of Ag and Li are quite near together. But the specific gravity of lithium was determined comparatively near the melting point of that metal, while that of silver was taken at a temperature greatly removed from the degree at which it fuses. Now *melted* silver, according to Playfair and Joule, has the specific gravity 9·206; and consequently an atomic volume of 11·7, nearly that of solid lithium. May not these two metals then, under similar circumstances, and at strictly comparable temperatures, be supposed to have equal atomic volumes?

O 2·6, 5·2, 10·4. S 10·4, 15·6. Se 10·4, 15·6.(?) Te 20·8. This value for S is that of the octahedral variety, prismatic

* This Journal for March and May, 1869.

sulphur having 16·3—16·7. Amorphous selenium has an atomic volume of 18·6.

Sr, 34·3. Ba, 34·2.

Ca, 25·8. Pb, 18·2.

As and P 12·9, Sb 17·2, Bi 21·5. These stand as 3 : 4 : 5. This value for P is that of the so-called "metallic" variety. Common phosphorus has an atomic value of 16·9—17·0, (perhaps 17·2, like Sb?) and the ordinary red modification 13·9—14·5. Amorphous arsenic has the value 15·9. Bo 4·1. Vanadium in my last paper I calculated theoretically from the atomic volume of one of its oxyds, making it equal to As and P. Since then, however, this idea has been overthrown, Roscoe having determined the specific gravity of the metal itself. This, 5·5, gives an atomic volume of 9·4, which differs widely from my supposititious value, and bears no definite relation which I can see, to those of As and P.

C (graphite), 5·5, Si 11·0, Ti 11·0 (?), Sn 16·5. These stand as 1 : 2 : 2 : 3. C (diamond) 3·4. Zr 21·7 (perhaps 22·0, or 5·5×4.)

Cr, Mn, Fe, Co, Ni, U,* and Cu,	6·9	These four values stand as 3 : 4 : 5 : 6.
Zn, Pt, Ir, Os, Pd, Ru, and Rh,	9·2	
Mo and W,	11·5	
Cd, Mg, and Hg, (solid)	13·8	

Gl 43,† Al 10·1—10·6, Th 30·4—30·9, Ce 16·7, In 10·2.

Before proceeding farther, it now becomes necessary for me to allude briefly to some regularities in atomic volumes which have already been traced by others. First, the alums have equal atomic volumes. To the evidence which has been cited by other investigators in proof of this, may now be added the atomic volumes of rubidium and cæsium alums, whose specific gravities, taken by Redtenbacher, show that they follow the regular rule. Second, a number of similar carbonates, especially those of Zn, Mg, Fe, and Mn, have equal atomic volumes. Third, a similar relation connects the vitriols with 7 aq. This series is especially important, since it contains the sulphates of Fe, Ni, Co, Mg, Zn, and Cd. Fourth, the oxyds allied to Gahnite, with the general formula MO, M_2O_3, are, with one or two exceptions, equal in atomic volume. Fifth, many corresponding phosphates and arsenates have equal values. Other series have been traced here and there, but these are perhaps the most striking.

Now, in the first place, we may lay it down as a general rule that when two similar elements have equal atomic volumes, the

* Possibly this may not belong in this group, but its equal atomic volume be merely a coincidence. The values of some of its compounds suggest this idea.

† Calculated from the lower atomic weight for Gl. In my last paper I used the higher.

atomic volumes of their corresponding compounds will also be equal. Of the truth of this I will cite a few examples, and point out some exceptions. For want of space I cannot adduce all the cases I have accumulated, however, but will merely attempt to illustrate the principle, and afterwards bring the exceptions under another rule.

Barium and strontium. $SrCl_2$, sp. gr. 2·8033, Karsten. $BaCl_2$, 3·704, Karsten. At. vols., 56·5 and 56·2.

SrI_2 4·415, Bödeker. BaI_2 4·917, Filhol. At. vols., 77·3 and 79·5. SrH_2O_2, 8 aq., 1·396 Filhol, BaH^2O_2, 8 aq., 1·656, Filhol. At. vols., each 190·2.

The sulphates and nitrates, bromids, carbonates, and simple hydrates, are either doubtful or exceptions. In all these cases the compounds of Ba have the highest values.

Iron group. Here examples are so abundant that I will only mention a few. MnO sp. gr., 5·38, P. & J.* NiO 5·597, P. & J. CoO 5·597–5·750, P. & J. UO 10·15, Ebelmen. CuO 6·130, Boullay. At. vols., respectively, 13·0, 13·4, 13·0–13·4, 13·4, 12·9.

Cr_2O_3 4·950, P. & J. Fe_2O_3 4·679–5·135, P. & J. NiO_3 4·814, P. & J. Co_2O_3 4·814, P. & J. At. vols., 30·7, 31·1–34·2, 34·4, 34·4. Mispickel, FeS_2, $FeAs_2$ 6·0–6·4 Gersdorffite, NiS_2, $NiAs_2$ 5·6–6·9. Cobaltite, CoS_2, $CoAs_2$, 6·0–6·3. At. vols., 50·9–54·3, 48·1–59·2, 52·6–55·3.

FeS 5·035. P. & J.: NiS 5·650, Rammelsberg; 4·601 Kenngott. CoS 5·45. At. vols., 17·5, 16·1–19 7, 16·7. Exceptions, MnS and CuS, having sp. grs. 3·95, Dana's Min., and 4·163, Karsten; and at. vols., 21·8 and 22·9. It is worth noting that these two sulphids stand at the ends of the series; CrS and US, being (if they exist), wholly unstudied in this direction.

One more striking exception may be found by comparing FeS_2, MnS_2, and CoS_2, whose atomic volumes are very dissimilar.

Platinum group. 2KCl, $PtCl_4$; and 2KCl, $IrCl_4$. Sp. grs. Bödeker, 3·586 and 3·546. At. vols., 136·2 and 137·9.

PdP_2 and PtP_2 have unequal values.

Molybdenum and tungsten. It has been proved by other authorities that MoO_3 and WO_3 have equal atomic volumes.

Cadmium group. CdO 6·9502, Karsten. HgO 11·344, P. & J. At. vols., 18·4 and 19·0. MgO has an exceptional value which will be studied in another connection.

Phosphorus and arsenic. Cu_3P 6·59, Hvoslef. Cu_3As 7·62, Genth. At. vols., 33·6 and 34·9.

P_2O_5 2·387, Brisson. As_2O_5 4·25 Filhol; 3·7342, Karsten. At. vols., 59·5 and 54·1–61·6.

* Playfair and Joule. This is the only abbreviation of the sort which I shall have occasion to employ. When I give no authority for a specific gravity, it will be found in "Dana's Mineralogy," last edition.

Phosphates and arsenates have been compared by others, Where their values differ, that of the arsenate is the highest.

Silicon and titanium. The acids of this group will be cited farther along. Fayalite, $2FeO\ SiO_2$ 4·006. $2FeO\ TiO_2$ 4·370 Hautefeuille. At. vols., 50·9 and 51·7.

The only definite exception is found in comparing $2KF\ SiF_4$; sp. gr. 2·6652, Stolba; and $2KF\ TiF_4$, 2 080, Bödeker. At vols., 82·5 and 116·3.

These examples will suffice for the present, although I shall have occasion to cite others to illustrate another point. Upon comparing all the material which I have collected, I find the exceptions to be quite rare, although, perhaps, I have given them undue prominence here.

Second, similar compounds of similar metals often have equal atomic volumes, even when those of the metals themselves are unequal. The corresponding compounds of the iron, platinum, and cadmium groups often exemplify this most strikingly. The exceptions to the previous rule are probably due to this. Thus, although Mg and Cd have equal atomic volumes, their oxyds show no such equality; that of the first metal having shaded off into approximate uniformity with those of its kindred iron group.

Of the rule under consideration, however, the most striking case hitherto adduced is that of the vitriols. As I have already stated, the sulphates with 7 aq. of Fe, Ni, Co, Zn and Mg, have equal atomic volumes, while the double salts of the same class, containing in addition to the above metals Cu and Cd, follow the same rule. A similar regularity connects the anhydrous sulphates of most of these metals. But an equally remarkable series is formed by the chlorids of the same metals, which, of the formula MCl_2, have, as far as they have been studied, atomic volumes which are very nearly equal. If we present these chlorids in tabular form, taking the average of their observed atomic volumes as the real value, we shall find that the variations from their average are wholly within the limits of experimental error.

$FeCl_2$.	Sp. gr. 2·528, Filhol.	At. vol., found, 50·2			Sp. gr. calc., 2·668	
$NiCl_2$	" 2·560, Schiff.	" " 50·7			" " 2·726
$CoCl_2$	" 2·937, P. & J.	" " 44·2			" " 2·726
$CuCl_2$	" 3·054, "	" " 44·0			" " 2·825
$ZnCl_2$	" 2·753, Bödeker.	" " 49·4	Mean, 47·6.		" " 2·857
$PtCl_2$	" 5·8696. "	" " 45·7			" " 5·638
$MgCl_2$	" 2·177, P. &J.	" " 43·6			" " 1·996
$CdCl_2$	" 3·6254, Bödeker,	" " 50·5			" " 3·844
$HgCl_2$	" 5·4032, Karsten,	" " 50·1			" " 5·693

Now, if we take into account the difficulty of obtaining perfectly accurate determinations of sp.gr. for some of these substances; and also bear in mind that for most of them only sin-

gle observations have been made, it is clear that every variation from equality may be ascribed to errors in experiment. The extreme difference in atomic volume is from a minimum of 43·6 to a maximum of 50·7. It is not rare for an equal divergence to occur between different determinations for a single substance.

Again, although chlorids, bromids, and iodids have unequal atomic volumes, those of similar chlorates, bromates, and iodates are equal,—at least as far as we have any data.

$NaClO_3$, sp. gr., 2·289, Bödeker. $NaBrO_3$, 3·339, and $NaIO_3$, 4·277, Kremers. At. vols., 46·5, 45·2, and 46·3.

$KClO_3$, sp. gr., 2·325, Buignet. $KBrO_3$, 3·271, and KIO_3, 3·979, Kremers. At. vols., 52·7, 51·1, and 53·8.

Most similar compounds of silver and sodium have equal atomic volumes. This holds true of the chlorids, bromids, and iodids (as will be shown in another connection hereafter), and of the sulphates, chlorates, nitrates, and probably carbonates. The sulphids and oxyds are exceptions.

Some compounds of As, Sb, and Bi, have equal values.

As_2O_3, sp. gr., 3·695, Guibourt; 3·884, Filhol. Sp_2O_3, 5·11, Terreil; 5·78, Boullay. Bi_2O_3, 8·079, P. & J.; 8·450, Leroyer & Dumas. At. vols., 51·0—53·6; 50·5—57·1; and 55·4—57·9.

In the carbon group compare certain oxyds.

SiO_2, 2·663, Deville. TiO_2 (artificial anatase) 3·700, Hautefeuille. SnO_2 6·720, Daubrée. At. vols., 22·5, 22·2, and 22·3.

Would space permit, I might go on multiplying examples to an almost indefinite extent; but my object at present is merely to illustrate certain principles. There are other regularities yet to be noticed. The first of these is comparatively unimportant, and I will merely state it as it is, without adducing any evidence. All sulphates have atomic volumes which are lower than the sum of the metallic oxyds and the SO_3. That is, the value for $MnSO_4$ is less than the sum of those of MnO and SO_3. To this rule I have found no exceptions. Whether a similar rule holds good for chromates, molybdates, and tungstates, I am not certain; the materials at my command being too limited to settle the question.

Another regularity is more remarkable. It is noteworthy that certain oxyds and sulphids have atomic volumes which are lower than those of the metals contained in them. Now, in a number of cases, I find that those atomic volumes equal *half* the sum of those of the metal and the other element, provided that S=15·6, and O receives its highest value, 10·4. I give oxygen this value, for this reason. Many sulphids have atomic volumes, as I have already stated, equal to the sum of those of

the metal and sulphur in the free state; so that it is probable that some oxyds follow the same rule. 10·4 being the highest number for oxygen, renders it likely that that is the true value for this element in those oxyds corresponding to the above mentioned sulphids.

Now, two sulphids appear to follow this rule, viz: Na_2S and K_2S, whose sp. gr., given by Filhol, are respectively 2·471 and 2·130. At. vols., 31·6 and 51·6. Now $\frac{2(22·8)+15·6}{2}=30·6$; and $\frac{2(45·6)+15·6.}{2}=53·4$. If these theoretical atomic volumes are true, then the sp. gr. of Na_2S will be 2·549, or 0·078 greater than the value found; while that of K_2S will be 2·059, varying 0·071 from Filhol's number. These variations are wholly within the limits of error for such compounds.

But the results obtained with four oxyds are more striking.

MgO.	Sp. gr. 3·200, Karsten.	At. vol. 12·5.	$\frac{13·8+10·4}{2}=12·1$.	Error 0·4
CaO.	" 3·161, "	" 17·7.	$\frac{25·8+10·4}{2}=18·1$.	" 0·4
SrO.	" 4·611, Filhol.	" 22·4.	$\frac{34·3+10·4}{2}=22·35$.	" 0·05
Al_2O_3.	" 3·928, Ebelmen.	" 25·9.	$\frac{2(10·2)+3(10·4)}{2}=25·8$.	" 0·1

The oxyds of barium, sodium, and potassium, do not follow this rule.

One more regularity traced, and I am done. If my views concerning multiple relations are correct, and all the values for oxygen and sulphur are multiples of the lowest, then we must expect that compounds formed by the union of these elements will have atomic volumes which are also multiples. Now, sulphuric anhydrid, SO_3, has, according to Buff, the sp. gr. 1·909 at 25°, and according to Baumgartner 1·975. Its atomic volume, then, is from 40·5 to 41·9. And 41·6 is precisely four times 10·4!

The chlorids, bromids, and iodids of the alkali metals and silver, seem to afford a similar example. In one of my previous papers I showed that Kopp's values for Cl, Br, and I, in their liquid compounds were almost exact multiples of his number for H, 5·5 Consequently, judging from analogy, it is likely that these elements in their solid compounds would follow a similar rule. Now the metals Li, Na, K, and Ag, have atomic volumes which do not vary greatly from multiples of 5·5. And, in accordance with what we should expect, their chlorids, bromids, and iodids have atomic volumes which are either exact, or so nearly exact, multiples of 5·5, that the circumstance cannot be ascribed to accident.

I present a tabular view of this regularity.

LiCl.	Sp. gr.	1·998, Kremers.	At. vol. found,	21·3.	Calc.,	22·0.	Sp. gr. calc.,	1·932		
NaCl	"	2·145, Buignet.	"	"	27·2	"	27·5	"	"	2·127
KCl	"	1·945, Kopp.	"	"	38·3	"	38·5	"	"	1·935
AgCl	"	5·130, Herapath.	"	"	27·9	"	27·5	"	"	5·218
NaBr	"	3·079, Kremers.	"	"	33·4	"	33·0	"	"	3·121
KBr	"	2·672, P. & J.	"	"	44·5	"	44·0	"	"	2·704
AgBr	"	5·8–6·0.	"	"	32·3–32·4	"	33·0	"	"	5·697
NaI	"	3·450, Filhol.	"	"	43·5	"	44·0	"	"	3·409
KI	"	3·056, Filhol.	"	"	54·3	"	55·0	"	"	3·018
AgI	"	5·350, Schiff.	"	"	43·9	"	44·0	"	"	5·341

On comparing these numbers, it will at once be noticed, not only that the variations from theory are remarkably slight, but also (which has been previously stated) that the corresponding compounds of Na and Ag have nearly equal, or equal atomic volumes, that the values of potassium compounds exceed those of the sodium compounds by 11·0, (5·5×2), that bromids exceed chlorids by 5·5, and that iodids have atomic volumes 11·0 greater than bromids. These regularities in difference between chlorids, bromids, and iodids, however, do not appear so distinctly in other series of them.

Now, to sum up the important relations traced in this paper, bearing in mind that in many cases exceptions exist.

First. When similar metals have equal atomic volumes, those of their similar compounds will also be equal.

Second. Metals whose atomic volumes are unequal, but simply related to one another, often form similar compounds having equal values.

Third. Some compounds have atomic volumes which stand in very simple relations to the sums of those of the free elements which form them.

Fourth. Compounds formed by the union of similar elements, have atomic volumes which are multiples of the lowest for that group.

And, fifth, we may add the multiple relations traced in my former papers, which not only connect the atomic volumes of different elements, but the various values for each single element.

Now, what do these regularities mean. Are we justified either in drawing any direct conclusions from them, or in basing upon them any general theory of atomic volumes? To this I must answer, that, although no generalization is absolutely established by them, it seems to me that one is decidedly hinted at. May we not say that in all compounds, the atomic volume of every element will be either a perfect multiple of the lowest value for that element, or of the lowest value in the group to which it belongs? Although this theory cannot be regarded as entirely proved, it certainly possesses a considerable degree of probability, and seems to harmonize well with the regularities which I have pointed out. But why an element should have a

higher value in one compound than in another, remains to be accounted for, although upon this point, perhaps, Buff's idea that the different degrees of quantivalence of an element in its various compounds, cause the differences in its atomic volumes, may prove correct.*

But at all events, whether the theory which I put forward turns out true or false, it may, perhaps, by lending some system to the study of atomic volume, pave the way for something of greater value.

Boston, May 30th, 1870.

ART. XIX.—*Considerations on the apparent inequalities of long period in the mean motion of the Moon;* by SIMON NEWCOMB.

[Read to the National Academy, April, 1870.]

THE problem of determining the motion of the moon around the earth under the influence of the combined attraction of the sun and planets has, more than any other, called forth the efforts of mathematicians and astronomers. Nearly every great geometer since Newton has added something to the simplicity or the accuracy of the solution, and, in our own day we have seen it successfully completed in its simplest form, in which the earth, the moon, and the sun are considered as material points, moving under the influence of their mutual attractions. The satisfactory solutions are due to the genius of Hansen and of Delaunay. Working independently of each other, each using a method of his own invention more rigorous than had before been applied, they arrived at expressions for the longitude of the moon which, being compared, were found to exhibit an average discrepancy of less than a second of arc. No doubt could remain of the substantial correctness of each.

The solutions here referred to exhibit only inequalities of short period in the motion of the moon. But, it has long been known, from observation, that the mean motion of the moon is subject to apparent changes of very long period, and especially to a secular acceleration by which it has been gradually increasing, from century to century, since the time of the earliest recorded observations. If we inquire into the problem of these inequalities of long period, we shall find it seemingly no nearer a final solution than it was left by La Place, observation having since added more anomalies than theory has satisfactorily shown to exist.

The first inequality in the order of discovery was the secular acceleration. This was discovered about the middle of the last century by a comparison of ancient eclipses with modern ob-

* See Buff's paper in the Annalen d. Chem. u. Pharm., 4th supplement vol., 1865–6. Or, see his "Grundlehren der theoretischen Chemie."

servations. Its cause was first discovered by La Place, who showed that it was due to the effect of the action of the planets in changing the eccentricity of the earth's orbit.

The results of his computations agreed substantially with observations, and was therefore received with entire confidence until less than twenty years ago. The question being then taken up by Mr. John C. Adams, this eminent mathematician was led to the conclusion that La Place's result was nearly twice too large.

The same conclusion was reached independently by Delaunay, and gave rise to a remarkable discussion, the history of which is too familiar to be now recounted. It is now conceded that the value found by Adams and Delaunay is theoretically correct.

The new result no longer agreeing with observation, the difference is now accounted for by an increase in the length of the day. That this length is increasing is also known from theoretical considerations, but the data for its accurate determination are wanting.*

In the third volume of the *Mécanique Céleste* (Seconde Partie, Livre vii, Chapitre v) La Place discusses an apparent inequality of long period in the motion of the moon. The discussion is mainly empirical. The existence of the inequality is inferred from observations, these showing that the mean motion of the moon during the half century following 1756 was less than during the half century preceding. He then assumed that the inequality was due to the fact that twice the mean motion of the moon's node, plus the motion of its perigee, minus that of the sun's perigee was a very small quantity, less than two degrees per annum, and determined the coefficient of the varying angle solely from the observations. The result was that these might be satisfied by supposing the inequality of mean longitude

$$\delta l = 47''\cdot51 \text{ [or } 15''\cdot39\text{] } \sin(2\,\Omega\,\mathbb{D} + \pi\,\mathbb{D} - 3\pi\odot)$$

If, in this expression, we substitute Hansen's values of the elements, it becomes

$$\delta l = 15''\cdot39 \sin[173^\circ\ 26' + (1^\circ\ 57'\cdot4)\ (t - 1800)].$$

When in 1811 Burckhardt constructed his tables of the moon,

* The time and place when the discordance referred to was first distinctly attributed to the tidal retardation of the earth having been a subject of discussion, the following extract from an article on "Modern Theoretical Astronomy" in the North American Review for October, 1861 (vol. 93, p. 385), may not be devoid of interest.

"It seems to be well established that the new theory is inconsistent with the observations of ancient eclipses, and if it should prove to be correct, we may be driven to the conclusion, that a portion of the acceleration proceeds from some other cause than the attraction of gravitation, or that the length of the day is gradually increasing to an extent which has become perceptible from the cause to which we have already referred [the tidal retardation, p. 374]. If, as centuries roll by, the day should gradually increase, the moon would move a little farther in the course of a day than if no such increase should take place. Since, in our calculations, we suppose the day constant, the apparent acceleration would be greater than the real—precisely the effect observed. The difference can be entirely accounted for by supposing an increase of something less than one thousandth of a second per century in the length of the day, and a corresponding diminution in the lunar month."

he omitted the sun's perigee from this argument by the authority of La Place, himself, who now attributed the inequality to a difference of compression between the two hemispheres of the earth. The function was also changed from *sin* to *cos* and the coefficient altered. The adopted term thus became

$$\delta l = -12''{\cdot}5 \cos [291^\circ\ 57' + (2^\circ\ 0'{\cdot}45)(t-1800)]$$
$$= \ 12''{\cdot}5 \sin [201^\circ\ 57' + (2^\circ\ 0'{\cdot}45)(t-1800)]$$

Succeeding investigators have regarded the theoretical coefficients of both of these terms as insensible. It does not seem likely that there is any such difference between the two terrestrial hemispheres as could produce the second, but I am not aware that the coefficient of the first has ever been shown to be insensible by any published computation. This coefficient is of the ninth order and the argument is,

In Delaunay's notation,	$3D-2F-l+3l'$;
In Hansen's,	$w-3w'$.

The period is 184 years, and the large value of the ratio of this period to that of the moon itself might render the coefficient sensible. Both Hansen and Delaunay pronounce it insensible, but neither publish their computations of its magnitude.

These terms have ceased to figure in the theory of the moon since Hansen announced that the action of Venus was capable of producing inequalities of the kind in question. So far as I am aware, Hansen's first publication on this subject is that found in No. 597 of the Astronomische Nachrichten (B. 25, S. 325.) Here, in a letter dated March 12, he alludes to La Place's coefficients, and says he has not been able to find any sensible coefficient for La Place's argument of long period. But on examining the action of Venus on the moon he found, considering only the first power of the disturbing force, the following term in the moon's mean longitude:

$$\delta l = 16''{\cdot}01 \sin (-g-16g'+18g''+35^\circ\ 20').$$

g, g' and g'' being the mean anomilies of the moon, the earth and Venus respectively. As this expression still failed to account for the observed variations of the moon's longitude he continued the approximation to the fourth power of the disturbing force, and found that the terms of the third and fourth order increased the coefficient to $27''{\cdot}4$, the angle remaining unchanged, so that the term became

$$27''{\cdot}4 \sin (-g-16g'+18g''+35^\circ\ 20'),$$

But this increase made the theory rather worse, and the term depending on the argument of Airy's equation between the earth and Venus was then tried with the result—

$$\delta l = 23''{\cdot}2 \sin (8g''-13g'+315^\circ\ 30').$$

The introduction of this term seemed to reconcile the theory with observation.

Hansen finally remarks that these values of the coefficients are still subject to some uncertainty from his not having employed decimals enough in his computation.

In a letter to the Astronomer Royal, published in the Monthly Notices of the Royal Astronomical Society for Nov. 1854, Hansen gives a statement of the elements employed in his tables of the moon, and refers to the subject of these inequalities in the following terms:—

"The accurate determination of these two inequalities by theory is the most difficult matter which presents itself in the theory of the moon's motion. I have on two occasions and by different methods sought to determine their values, but I have obtained results essentially different from each other. I am now again engaged with their theoretical determination by a method which I have simplified, and hope to bring the operation to a definitive close. I have also applied to my tables some coefficients which are not free from empiricism but which I can justify by the circumstance that they represent the ancient as well as the modern observations with great exactness, and it may be expected that they will represent the future observations equally well."

Hansen's lunar tables were published in 1857.

The terms of long period finally adopted are

$$15''{\cdot}34 \sin (-g-16\mathrm{E}+18\mathrm{V}+30^\circ\ 12')$$
$$+21{\cdot}48 \sin (8\mathrm{V}-13\mathrm{E}+274^\circ\ 14'),$$

V and E representing the mean longitudes of Venus and the earth. Changing them to mean anomalies the terms become

$$15''{\cdot}34 \sin (-g-16g'+18g''+33^\circ\ 36')$$
$$+21{\cdot}47 \sin (8g''-13g'+4^\circ\ 44').$$

It appears that while the first term has been restored to what was substantially its original value, when only the first power of the disturbing force was included, the argument of the second term has been changed by 50°, the coefficient being but slightly changed.

In a letter to the Astronomer Royal, dated 1861, Feb. 2d, found in the Monthly Notices for March, 1861, Hansen again refers to this second term with the statement that its coefficient is one of those somewhat empirical. At the same time he has found the coefficient, by his last theoretical determination of it, by no means insensible, like Delaunay. He adds that in the comparison with observation he has never gone beyond Bradley, nevertheless his tables satisfactorily represent the ancient observations.

A well marked feature of Hansen's published works is the copiousness and perspecuity with which his theoretical calculations are laid down. But, so far as I am aware he has never published any computation of these inequalities except that part of the first inequality which depends on the first power of

the disturbing force. This computation is found in vol. xvi of the Memoirs of the Royal Astronomical Society. In the second part of his "Darlegung" we find a general method of treating inequalities of long period, but—unless I have overlooked it—no computation of any particular inequality. Nor do we find any statements of the numerical results of Hansen's various computations except those already quoted.

The only geometer besides Hansen who has attacked the problem of these inequalities is Delaunay. His researches are published in full in the Additions to the Connaissance des Temps for years 1862 and 1863. For the first approximation to the first inequality his result is

$$16''{\cdot}02 \sin (-l-16l'+18l''+35^\circ\ 20'{\cdot}2)$$

a result practically identical with that of Hansen. The ulterior approximations change it to

$$16''{\cdot}34 \sin (-l-16l'+18l''+35^\circ\ 16'{\cdot}5),$$

so that they increase the coefficient instead of diminishing it as in Hansen's theory. The difference is however so small that the results may be regarded as identical.

But, in the case of the second inequality instead of reproducing the result of Hansen, he finds a coefficient of only $0''{\cdot}27$, a quantity quite insignificant in the present state of the question. We have thus an irreconcilable difference on a purely theoretical question.

I propose to inquire whether we have in either theory an entirely satisfactory agreement with observation. As a preliminary step to this inquiry I have prepared the following table of the mean longitude of the moon from the tables of Burckhardt and of Hansen respectively, for a series of equidistant dates, the interval being 3652·5 days, and the epoch 1800 Jan. 0, Greenwich mean noon. These dates are marked by the year near the beginning of which they fall. Column L_0 gives Burckhardt's mean longitude on the supposition of uniform motion, from the data given on the fifth page of the introduction to his tables. Next is given the acceleration of the mean longitude deduced from Table XLVIII. The inequality of long period is from Table XLIX. The sum of these three quantities gives the corrected mean longitude.

Hansen's mean longitude and secular acceleration are deduced in the same way from the elements given on page 15 of his *Tables de la Lune*. His terms of long period are deduced from Tables XLI and XLII, the constants being subtracted and the remainder reduced to arc by being multiplied by the factor $0''{\cdot}004703$. The last column of the table gives the correction to Burckhardt's mean longitude to reduce it to that of Hansen. That this difference is really the mean difference between the longitudes of the moon deduced from the two tables is shown

by its agreement with the known difference at particular epochs. At the end of the British Nautical Almanac for 1862 is found a comparison of the two tables, from which it appears that Burckhardt's mean longitude was then greater than Hansen's by about 14″·2. The general agreement between 1750 and 1800, when both tables agreed with observations, shows that the difference of mean motion is certainly affected with no sensible error.

Year.	Burckhardt.							Hansen.							H.–B.
	L_0			Sec. Var.	Long Period.	Corr. Mean Longitude.		L_0			Sec. Var.	Long Period.	Mean Longitude.		
	°	′	″	″	″	°	′	″	″	′	″	″	″	°	′	″	″
1630	100	19	28·0	+ 4·9	— 8·0	100	19	24·9	18	14·4	+ 38·5	— 21·4	100	18	31·5	—53·4
40	347	6	45·4	+ 3·6	—10·8	347	6	38·2	5	36·3	+ 34·1	—20·0	347	5	50·4	—47·8
50	233	94	2·7	+ 2·5	—12·3	233	53	52·9	52	58·3	+ 30·0	—17·2	223	53	11·1	—31·8
60	120	41	20·1	+ 1·6	—12·3	120	41	9·4	40	20·3	+ 26·1	—13·1	120	40	33·3	—36·8
70	7	28	37·4	+ 0·9	—10·8	7	28	27·5	27	42·2	+ 22·5	— 8 1	7	27	56·6	—30·9
80	254	15	54·8	+ 0·4	— 8·0	254	15	47·2	15	4·2	+ 19·2	— 2·3	254	15	21·1	—26·1
90	141	3	12·1	+ 0·1	— 4·2	141	3	7·8	2	26·1	+ 16·1	+ 3·9	141	2	46·1	—21·7
1700	27	50	29·5	+ 0·0	+ 0·2	27	50	29·7	49	48·1	+ 13·3	+ 10·0	27	50	11·4	—18·3
10	274	37	46·8	+ 0·1	+ 4·4	274	37	51·3	37	10·0	+ 10·8	+ 15·6	274	37	36·4	—14·9
20	161	25	4·2	+ 0·4	+ 8·3	161	25	12·9	24	32·0	+ 8·5	+ 20·5	161	25	1·0	—11·9
30	48	12	21·5	+ 0·9	+ 11·0	48	12	33·4	11	59·9	+ 6·5	+ 24·2	48	12	24·7	— 8·7
40	294	59	38·9	+ 1·6	+ 12·4	294	59	52·9	59	15·9	+ 4·8	+ 26·4	294	59	47·1	— 5·8
50	181	47	56·2	+ 2·5	+ 12·2	181	47	10·0	46	37·9	+ 3·3	+ 26·9	181	47	8·1	— 2·9
60	68	34	13·6	+ 3·6	+ 10·6	68	34	27·8	33	59·8	+ 2·1	+ 25·7	68	54	27·6	— 0·2
70	315	21	30·9	+ 4·9	+ 7·8	315	21	43·7	21	21·8	+ 1·2	+ 22·9	315	21	45·9	+ 2·2
80	202	8	48·3	+ 6·4	+ 3·9	202	8	58·6	8	43·7	+ 0·5	+ 18·5	202	9	2·7	+ 4·1
90	88	56	5·6	+ 8·1	— 0·4	88	56	13·4	56	5·7	+ 0 1	+ 12·8	88	56	18·6	+ 5·2
1800	335	43	23·0	+ 10·0	— 4·7	335	43	28·4	43	27·7	0·0	+ 6·1	335	43	33·8	+ 5·4
10	222	30	40·4	+ 12·1	— 8·3	222	30	44·2	30	49·6	+ 0·1	— 1·1	222	30	48·6	+ 4·4
20	109	17	57·8	+ 14·4	—11·0	109	18	1·2	18	11·6	+ 0·5	— 8·4	109	18	3·7	+ 2·5
30	356	5	15·2	+ 16·9	—12·4	356	5	19·7	5	33·5	+ 1·2	—15·4	356	5	19·3	— 0·4
40	242	52	32·5	+ 19·6	—12·2	242	52	39·9	52	55·5	+ 2·1	—21·6	242	52	36·0	— 3·9
50	129	39	49·9	+ 22·5	—10·6	129	40	1·8	40	17·5	+ 3·3	—26·5	129	39	54·3	— 7·5
60	16	27	7·2	+ 25·6	— 7 6	16	27	25·2	27	39·4	+ 4·8	—29·8	16	27	14·4	—10·8
70	263	14	24·6	+ 28·9	— 3·8	263	14	49·7	15	1·4	+ 6·5	—31·3	263	14	36·6	—13·1

Burckhardt's tables have been selected for this comparison because they have been extensively compared with observations made before 1700. The additions to the Connaissance des Temps for 1824 contain a paper by Burckhardt himself giving a comparison of his tables with observations of occultations made by Flamstead, Hevelius and others, between 1637 and 1700. The general result of this comparison is that the mean longitude of his tables could hardly have been more than a very few seconds in error in the year 1670. But, the preceding table shows that for this epoch Hansen's mean longitude is 30″ less than Burckhardt's. Therefore, unless we suppose Burckhardt's investigation to be affected with some egregious systematic error we must admit that the mean longitude of Hansen's tables for the epoch 1670 is about 30″ too small.

Desiring an independent test of this conclusion I have selected certain observations which, with the data available, seemed

well fitted to answer this purpose and compared them directly with Hansen's Tables.

They are

1. Occultation of Aldebaran, 1680, Sept. 13, observed at Greenwich by Flamstead.

2. Occultation of the same star 1680, Nov. 7, observed at Greenwich by Flamstead, and at London by Halley.

3. Total eclipse of the sun 1715, May 3, observed at London, Greenwich and Wanstead by Halley, Flamstead and Pound.

To compute the occultations of Aldebaran the mean position for 1680·0 was derived from Le Verrier's Tables (Annales de l'Observatoire, Tome II) correcting the right ascension by $+0^s.01$, and was as follows:

$$\alpha(1680) = 4^h\ 17^m\ 37^s.01$$
$$\delta \ldots . +15^\circ\ 49'\ 11''.8$$

The corrections for reduction to apparent place are

for Sept. 13, $\Delta\alpha = +2^s.90$; $\Delta\delta = +1''.1$
Nov. 7, $\Delta\alpha = +4^s.18$ $\Delta\delta = +2''.4$

The following geocentric positions of the moon were derived from Hansen's Tables.

Date (Julian Cal.)	Sept. 13.						Nov. 7.					
	h	m	s	h	m	s	h	m	s	h	m	s
Gr. Mean Time,	15	0	53	16	12	53	7	50	39	8	48	15
☽'s Longitude,	64°	54′	24″·3	65°	37′	20″·4	64°	33′	11″·6	65°	9′	49″·6
" Latitude,	−4	45	29·8	−4	48	10·6	−4	39	26·9	−4	40	48·0
" Parallax,	0	59	30·0	0	59	28·8	1	1	18·5	1	1	17·8

From these data we derive the following times for the immersion and emersion of Aldebaran for the dates in question. The observed times have been concluded from the observed altitudes and clock times given by Flamstead in the *Historia Celestis*, kindly furnished me by Prof. Winlock. They differ but little from the results of Flamstead himself, when the latter are corrected for the equation of time.

	Computed.			Observed.			C—O.
	h	m	s				s
Sept. 13, Immersion,	15	2	49	15	0	53	+116
Emersion,	16	10	5	16	9	12	+ 53
Nov. 7, Immersion,	7	51	47	7	50	43	+ 64
Emersion,	8	48	16	8	47	12	+ 64

The great difference between the results of the two phases of the first occultation gives rise to a suspicion of error in the observations or the data of reduction. The second observation is confirmed by that of Halley in London, he having observed the immersion at $7^h\ 50^m\ 9^s$, and noticed that the star was "newly emerged" at $8^h\ 47^m\ 1^s$. His place of observation was probably twenty-five or thirty seconds west of Greenwich, and there-

fore his observation agrees well with that of Flamstead. The discordance between the observed and computed times, of this second occultation indicates a correction of about $+32''$ to Hansen's mean longitude at the epoch 1680, and the first may be considered as confirming this correction in direction, if not in amount.

For the eclipse of May 3, 1715 we have the following computed and observed times. I have assumed Halley's station to be in latitude 51° 31′ and longitude 25^s west. Pound's is taken in accordance with his own statement to be in latitude 51° 34′, and longitude 8^s west. These agree pretty well with Flamstead's statements that Wanstead is seven or eight miles N. by E. from Greenwich,* and that Crane Court is half a minute of time West of Greenwich.

Halley at London.

	Computed.			Observed.			C−O
	h	m	s	h	m	s	s
First contact,	20	2	35	20	2	37	− 2
Beginning of Totality,	21	5	52	21	5	39	+13
End of "	21	9	3	21	9	2	+ 1
End of Eclipse,	22	16	55	22	16	37	+18

Pound at Wanstead.

	Computed.			Observed.			C−O
	h	m	s	h	m	s	s
Eclipse first perceived,	20	3	18	20	3	15	+ 3
The total immersion,	21	6	38	21	6	6	+32
The emersion,	21	9	48	21	9	26	+22
The just end of the eclipse.	22	17	42	22	17	10	+32

The only information I have respecting Flamstead's observations is contained in a letter of his found in Baily's 'Life and Correspondence of Flamstead, p. 315, from which it appears that his times differ only a few seconds from Halley's, instead of differing by the half minute required by the difference of meridians. An obvious slip of the pen, (*later* being written instead of *earlier*) makes it doubtful in which way the "few seconds" are to be counted. It can, however, be fairly inferred from his statement that his observations diverge from the tabular times as much or more than Pound's.

The discordance of the results of first and last contact may be attributed to this cause: that with their imperfect telescopes the observers did not begin to see the moon until several seconds after the actual commencement of the eclipse, and lost sight of it a few seconds before the actual end. The discordance in the duration of totality indicates with a high probability that the computed shadow path falls a few miles too far north. In this case the mean of the results for beginning and end of totality

* Baily's Flamstead, p. 316, p. 328.

will be about right, and we have for the excess of computed times

Halley's observations,	$+7^s$
Pound's,	$+27$
Flamstead's,	$+30\pm$

I infer from these results that the correction to Hansen's mean longitude at the epoch 1715 is about $+11''$.

Comparing the corrections thus found for the epochs 1680 and 1715, we find they are substantially those required to reduce Hansen's mean longitude to Burckhardt's. I conclude, therefore, that no egregious systematic error has crept into the researches by which Burckhardt sought to show that the epoch of his tables was substantially correct during the latter half of the seventeenth century, and that the difference between the mean longitude of Hansen and Burckhardt during that period represents approximately, at least, errors of Hansen's mean longitude.

The observations of the moon made at the observatories of Greenwich and Washington during the last ten years, indicate a tabular deviation of a remarkable character. From 1850 to 1862 we find the moon slowly running ahead of the tables, until the latter required a correction of plus two seconds in longitude to make them agree with observation. But this correction, instead of continuing to increase as all analogy would have led us to anticipate, suddenly began to diminish, so that since 1862 the moon seems to have been falling behind the tables at the rate of a second a year. This is shown by the following exhibit of the corrections to Hansen's mean longitude, or right-ascension, deduced from the meridian observations of the two observatories.

Year.	Correction given by Greenwich.	Washington.	Mean.	Corr. mean.
	$''$	$''$	$''$	$''$
1850	+0·3	−1·3	0·0	+1·0
51	+1·5	+0·6	+1·3	+2·7
52	+0·9	----	+0·9	+2·4
56	+1·0	----	+1·0	+1·4
57	+1·5	----	+1·5	+1·4
58	+2·0	+1·5	+1·8	+1·3
62	+2·4	+2·4	+2·4	+0·9
63	+2·2	+1·2	+1·7	+0·5
64	+0·1	−1·0	−0·4	−1·2
65	−1·1	−2·4	−1·7	−2·1
66	−2·2	−2·5	−2·4	−2·4
67	−3·9	−4·1	−4·0	−3·6
68	−4·4	−4·5	−4·5	−3·6
69	----	−5·5	−5·5	−4·3

The corrections here given as those of Greenwich are, previous to 1859, derived from the comparison found in the Green-

wich observations for 1859. From 1863 forward they are derived from a paper by Mr. Dunkin in the Monthly Notices of the Royal Astronomical Society for April, 1869. The work of only the four principal observers is therefore included in the comparison. The object of this comparison being not so much to determine the absolute correction to the epoch of the tables as to show the changes of this correction, it is better to reject the results of the observers whose labors were discontinuous. In the case of the Washington observations, such a selection could not be made: the results given are therefore an indiscriminate mean of all. The systematic personal differences are however found to be very small.

That these corrections are real will not, I conceive, be disputed. To suppose them due to errors of observation, would be to suppose that six or eight long practiced observers divided between the two hemispheres, all progressively changed their habits of observing in the same way, and to nearly the same amount, through a period of seven or eight years.

A portion of the observed discordance may arise from a small error in Hansen's value of the coëfficient depending on the ellipticity of the earth, which is more than a second greater than the values derived by previous investigators, either from theory or observation. The last column of the preceding table shows what the correction would be if Hansen's coëfficient were 1″·5 smaller than it is.

From all these comparisons it would appear that the problem of the inequalities of long period in the moon's mean motion is really no nearer such a solution as will agree with observation, than when it was left by La Place. By a partially empirical correction, Hansen has succeeded in securing a very good agreement during the period 1750–1860, but, if the results of the preceding examination are correct, this has been gained only by sacrificing the agreement for the century previous to 1750, and for the years following 1860. This failure to reconcile theory with observation must arise from one of two sources. Either:

(1) The concluded theory does not correctly represent the mean motion of the moon. Or:—

(2) The rotation of the earth on its axis is subject to inequalities of irregular character and long period.

The first hypothesis admits of two explanations. We may suppose either that the mean motion of the moon is subject to change from some other cause than the gravitation of the known bodies of the solar system, or that the effect of this gravitation is incorrectly calculated, and that theory and observation will be reconciled by a correct calculation.

There are difficulties in the way of accepting either of these explanations. In reference to the first it may be remarked that

anomalies of mean motion cannot be accounted for by a deviation from the received law of gravitation inversely as the square of the distance, because the anomalies produced by such deviation would be regularly progressive, and would be most sensible in the secular motion of the moon's perigee. The comparison of the theoretical and observed values of this motion is, perhaps, the severest test to which the Newtonian law has yet been subjected. That the anomalies proceed from the attraction of unknown bodies passing through the system seems extremely improbable, since, if they were distant, they would affect the earth and planets more than the moon, while the closer passage of bodies could scarcely escape detection. Still, this explanation does not admit of being mathematically disproved. If we attribute the deviation to the impact of meteoric matter, we must suppose the moon to have encountered such matter in quantities nearly incredible.

These three causes exhaust those on which we can base the first explanation, unless we invalidate the third law of motion. For, by that law, matter moves only by the influence of other matter. Other matter can affect the motion of the moon only by impact and gravitation. The gravitation of known bodies. the gravitation of unknown bodies, and the impact of matter is therefore an exhaustive enumeration.

We pass now to the second explanation of the first hypothesis, namely, errors or omissions in the theoretical computation of the effect of gravitation. The wide difference between the conclusions of Hansen and Delaunay suggests the possibility that there may be inequalities still overlooked. We have however the assurance of Hansen that there are none, and we shall find it extremely difficult to introduce any periodic terms whatever which will represent the observed deviation of the moon from the tables during the past ten years, without discordance during the century previous, when the agreement of Hansen's tables with theory is believed to be quite close. It is however hardly worth while to dwell upon this explanation until we have a more rigorous theory of the inequalities of long period produced by gravitation.

Considering that the reconciliation of theory and observation is not very probable, the second hypothesis may become worthy of serious consideration. If we accept it we must admit that between the years 1860 and 1862 the rotation of the earth was so accelerated that our reckoning of time is already eight or ten seconds ahead of what it would have been had the day remained invariable. Such an acceleration could proceed only from a change in the arrangement of the matter of the earth. The possibility of this effect being produced by changes in the quantity of ice accumulated around the poles has, I be-

lieve, been pointed out by geologists. But the effect of this cause could scarcely be sensible. But, if we admit that the interior of the earth is a fluid, and also admit that general changes in the arrangement of this fluid are possible, we have all that is necessary to account for considerable changes in the rotation of the outer crust. That this fluid, admitting its existence, is not in a state of entire quiescence is rendered probable by the phenomena of volcanoes and earthquakes. If we suppose a large mass of it to move from the equatorial regions to a position nearer the axis, a mass from the latter position taking its place, the following effects will follow:—

1. A diminution in the angular velocity of the surface of the fluid, accompanied by a corresponding increase in the velocity of the axial portion. The velocity of the outer crust will then be gradually retarded by friction.

2. The gradual transmission of the increased rotation of the central mass to the surface by friction and viscosity. The motion of the crust will then be gradually accelerated. The velocity of rotation finally attained will be greater or less than the original velocity, according as the radius of gyration of the fluid mass is diminished or increased by the change in the arrangement of the fluid.

I conclude, from this discussion, that we have reason to suspect that the motion of rotation of the crust of the earth is subject to inequalities of an irregular character, which, in the present state of science, can be detected only by observations of the moon. This suspicion can be neither confirmed nor removed until we have more positive knowledge than we now have of the possible inequalities which may be produced in the mean motion of the moon by the action of gravitation.

The operation of calculating these inequalities, though complicated and difficult, is certainly within the powers of analysis. When it is completely and thoroughly done, we may ascertain whether the result can be made to represent observations. If so, well; the length of the day is not variable, and the future positions of the moon can be safely predicted. If not, it will follow either that the motion of the moon is affected by other causes than the gravitation of the known bodies of the solar system, or the day is irregularly variable.

By the end of the present century, if not sooner, we shall have an independent test of the latter hypothesis, in the agreement of the observed and theoretical times of the transits of Mercury and Venus. If the hypothesis is a true one, the irregularities may range over half a minute of time in the course of a century, and this quantity might be detected even by meridian observations of the planets in question.

ART. XX.—*Researches in Electro-Magnetism*; by ALFRED M. MAYER, Ph.D.

THE refined experiments of Weber, Tyndall and Knoblauch having fully established the fact of the reversed polarity of a bar of bismuth, when subjected to the magnetizing influence of a helix, the attention of natural philosophers is naturally directed to the necessity of subjecting Ampère's theory of magnetism to as severe deductive tests as can be applied. Thus, from new experiments and lines of research suggested by theoretic deduction, we may hope for such addition to our knowledge as will evolve a theory which will embrace both magnetic and diamagnetic phenomena, as completely as Ampère's beautiful generalization contains the explanation of the magnetization of iron.

Among other experiments, thus suggested, was the following: Ascertain the relative forces of two electro-magnetic cores; one composed of *insulated* wires, the other of the same number of similar wires *uninsulated*.

Theory indicates that the insulated bundle will be found the weaker; for, in this case, we have not only the reaction of the molecular currents, but also, the reaction of the infinitely larger currents flowing around each insulated wire. Also, admitting the truth of the molecular hypothesis, we have in the uninsulated wire-core the interaction of exceedingly small currents separated by distances, great when compared to their size; while in the insulated wire-core we have, in addition, very large currents reacting at distances very small when compared to their diameters.

It was the attempt to solve this problem which led to the invention of the experimental method described in this paper; for, it will appear further on, that it could not have been attacked by methods heretofore used. The satisfactory solution of this question and the ascertained delicacy and precision of the apparatus encouraged me to make other determinations, which I here present; not as a finished and neatly rounded piece of work, but as showing what may be expected from a more complete discussion of the method used, and from that perfect experimental control of the apparatus which a more extended experience will give.

In my first experiments I adopted the plan of Müller (Poggendorff's Annalen, Bd. LXXIX and LXXXII) and tried the forces of the different cores, magnetized in one and the same helix, by their action on a distant magnetic needle; keeping the current, as far as possible, of the same intensity during the two comparisons. This method I found could not serve the purpose of measuring forces differing but slightly in intensity; and the

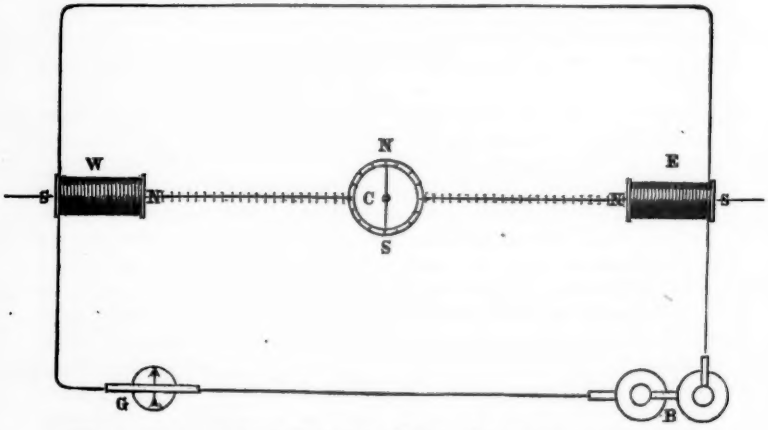
impossibility of obtaining a current so constant as not to produce continual motion and vibration of the needle caused me to devise the following method, which I have found from experience to be both sensitive and precise.

Apparatus for the comparative measures of electro-magnetic forces.—On a table 10 ft. long was drawn a center line and divided into fractions of an inch. This line was then accurately placed at right angles to the magnetic meridian. Two helices, which I designate as E and W, were placed 8 ft. apart with their axes in the same vertical plane as the above line. A surveyor's compass, with a sensitive needle 5·85 in. long was so arranged that the point of suspension of the needle could be moved between the helices in the line of their axes. The same battery current passed through both helices, and in such direction that the N. pole of each was facing the needle; by reversing the current, the S. poles could be opposed to each other.

Both helices were composed of 10 layers of ·1 inch "extra covered" copper wire, wrapped on copper spools of $8\frac{5}{8}$ inches long, 1·82 inches diam., and having flanges at the ends 1·25 inches high. These spools, with their flanges, were split in the direction of their length by an opening of $\frac{1}{16}$ inch. Each layer of coils was saturated with a thick solution of shellac in alcohol and covered with thick paper, coated with shellac, before the succeeding layer was wrapped.

Helix E contains 557·5 feet of wire in 696 turns, and on account of its better insulation and greater number of turns is superior in strength to helix W. Helix W contains 551 feet of wire in 688 turns.

1.



The accompanying diagram shows the arrangement of the apparatus. Helix E to the east of the compass; helix W to the

west; at C the compass; and at G the tangent galvanometer sufficiently removed from E and W, not to be affected by the magnetized cores. The Bunsen battery is shown at B.

Each end of the compass needle is thus subjected to the powerful action of two opposing forces and a magnetic couple is thus formed whose equilibrium, shown by the needle, is readily disturbed by a change in the relative forces of the cores or by a change in the distance of the needle relatively to the two helices.

It was found that when the needle was placed at such a position on the line between the helices that it stood at 0° when the circuit was open or closed, and a current passed from 12 cells of a Gaiffe bichromate battery, which was rapidly decreasing in electro-motive force, the needle remained at 0° for over 20 minutes without change or tremble.

Now if we can find the law which shows the variation of the intensity of action of the electro-magnet on the needle with a change of its distance from the magnetized core, we will have a system of measurement for the electro-magnetic forces which will exceed in accuracy and in delicacy, though be similar in arrangement, to the best photometric processes.

The advantages of this method, as far as my knowledge of it extends, is that you can thus subject the needle to two opposing actions of great intensity and thus any minute difference in the causes which alter their relative intensities will, on account of the nearness of the needle to the helices, give a deflection which would be inappreciable in the methods heretofore used; and above all, the same current traversing both helices will, even if inconstant and fitful, affect both proportionately and the needle, as seen above, will preserve a fixed position at 0° under these circumstances.

The law of variation of the intensity of the force with a change of distance.—To find the law of variation of the force with the distance from the pole of the core (which as is well known exists in the plane of the end-coils of the helix) the following experiments were made.

A core 10 in. long and 1·64 in. in diameter composed of 400 soft well annealed iron wires, was placed in helix E and the plane of the ends of the wires facing the needle were brought into the plane of the end coils of the helix. The center of the compass needle was placed at distances of 4, 5, 6, 7 and 8 ft. from the end of the core; the needle brought to 0° at each of these positions by the action of terrestrial magnetism and the current passed from seven new, freshly amalgamated Bunsen-cells; as soon as the needle had come to rest the angle of deflection was read and at the same time the tangent galvanometer was noted.

The following table gives the results, each number being the mean of four experiments.

Column (1) gives the distance of the center of needle from core; (2) the readings of the tangent galvanometer; (3) the deflection of the compass needle for the distances given in column

(1) Dist. from Core.	(2) Tan. Gal.	(3) Deflection.	(4) Nat. Tan. Defl.	(5) $\frac{1}{d^3}$	(6) $\frac{1}{d^{2\cdot7404}}$	(7) Dif.
4 ft. or 1	51° 45′	38° 15′	·788336	·788336	·788336	·000000
5 " " 1·25	51° 45′	23°	·424475	·829042	·782392	·005944
6 " " 1·5	51° 30′	14° 30′	·258618	·872835	·785629	·002707
7 " " 1·75	51° 45′	9° 45′	·171831	·917888	·796385	·008049
8 " " 2	51° 30′	6° 44′	·118063	·944504	·789144	·000808

(1); the nat. tangs. of the angles of (3); (5) the products of the nat. tans. of the angles of deflection by the cubes of the distances in (1); these products being obtained by multiplying the nat. tan. of each angle of deflection by the corresponding distance from the core raised to the cube, should, if the law followed the reciprocals of the cubes, be equal; column (6) gives the products of the nat. tangs. of the needle deflections into their corresponding distances from the core raised to the 2·7404 power; (7) contains the differences of each product of (6) from ·788336 the nat. tan. of 38° 15′. If the experiments had been more extended these differences would have been smaller; as it is, they are not great, and greater approximation cannot be obtained with three decimals in the exponent of the power, and this degree of accuracy is all the determinations will allow. The sum of the + departures equals ·008857, that of the − departures equals ·008651, their difference is +·000206.

We can therefore assume that *the intensity of the force* (for this apparatus) *varies inversely as the* 2·7404 *power* (*or as* $d^{2\cdot7404}$) *of the distance from the core.*

Experiments on the comparative strengths of cores formed of insulated and uninsulated wires.—The wire used in these experiments was of the best soft iron and of two diameters, $\frac{1}{16}$ and $\frac{1}{32}$ inch. The wire was cut into lengths of 10 in. and then placed in iron tubes, (to keep them from the action of the air), and heated to bright redness; the tubes were then covered thickly with lime and allowed to cool slowly. This process was twice repeated on all the wire and iron tubes and cylinders used in this investigation, and they were found after magnetization to have very little residual magnetism. The caution however was taken always to present the same end of a core toward the needle; thus all the cores were placed in the same circumstances. Three cores were made of each size wire. Each core of the $\frac{1}{16}$ wire contained 100 pieces, while each core of the $\frac{1}{32}$ wire consisted of 400 wires. The wires of one core of each size wire, (after having been tested, as in Ex. 7, that they were equally

affected in the same circumstances of magnetization), were insulated by dipping them separately into parafine, melted in a tube about 12 in. long, and were thus uniformly coated with a firm layer of that substance. They were then closely tied together into bundles by means of silken thread. The helices, in all the experiments on the cores, were placed 8 feet apart.

To abbreviate the descriptions of the experiments we will always place the E helix east of the compass and the W helix west, and to indicate the deflection of the needle we will use such expressions as "S. 17° W." which means, S. *end* of *needle* deflected 17° W. or toward helix W. In the case of N. poles facing the compass the above expression would show that the core of helix W was stronger than that of helix E. If the current was reversed it would indicate that the E helix was the stronger. When not otherwise stated it will be understood that the N. poles of the helices face the needle, and therefore the expression given of the deflection will always show which of the cores has the greater action on the needle. To designate the different wire-cores we will use "100 or 400 UC" or "100 or 400 I C," which respectively mean, *the uninsulated core* of 100 or 400 wires and *the insulated core* of 100 or 400 wires. T. G. stands for tangent galvanometer.

Experiment 1. Helices alone, without cores. Needle placed midway between helices and brought to 0°. Current passed from 10 Bunsen cells. Needle S. 4° E. T. G. 58°.

Ex. 2. Reversed current S. 4° W. T. G. 58°.

Ex. 3. Moved needle toward W. helix so that it stood at 0° when circuit was open or closed. Distance from E helix = 52·77 in. T. G. 58. Expts. 1, 2 and 3 show that E helix is the stronger.

Ex. 4. Three cores of 400 wires each, one of insulated wires, were compressed to exactly the same diameter of 1·64 ins. An uninsulated core was placed in each helix, and the needle brought to 0°, midway between the helices. Current passed S. 27° E. T. G. 50°.

Ex. 5. Current reversed S. 27° W. T. G. 50°.

Ex. 6. Other circumstances remaining as in (4) moved needle toward W helix until it stood at 0° when circuit was open or closed. Needle distant from E helix 55·6 in. T. G. 46°. Expt. repeated many times, with current direct and reversed, with the same result.

Ex. 7. Interchanged the cores in the helices. Current passed. Needle still at 0°. T. G. 48°. Current reversed. Needle at 0°. This shows that the cores were equally affected in the same circumstances.

Ex. 8. Replaced E core by the *insulated* core of 400 parafined wires. Needle having remained in same position as in (6) and (7). Current passed. S. 55′ W. T. G. 46°.

Ex. 9. Current reversed S. 55′ E. T. G. 46°.

Expts. 8 and 9 repeated several times with same results, showing that the insulated core was the weaker.

Ex. 10. Moved needle toward E helix, which contained the 400 insulated wires, until it stood at 0° when circuit was open or closed. In this position needle was 55·4 in. from E helix.

The 100 wire-cores substituted for the 400 wire-cores gave similar results only differing in degree.

It is very important to remark that errors may be introduced into such determinations as the above, by not taking care to have the various cores which we compare of *exactly the same diameter*. To show the effect of a change of diameter in the same bundle of wires the following experiment was made.

Ex. 11. The two cores of 400 uninsulated wires of 1·64 in. diameter were placed in the helices and needle brought so that it stood at 0° when circuit was open or closed. The E core was now so firmly compressed that its diameter was reduced to 1·48, and replaced in E helix, otherwise the circumstances were the same. Current passed. S. 1° 15′ W., showing a considerable diminution in the magnetic force of the compressed core; and also, beautifully exhibiting the *surface* action of magnetic force.

It will be observed, in the above comparative experiment, that the two uninsulated wire-cores were of exactly the same strength when equally magnetized, (Ex. 7.) and that one of them remained in the W helix as *unit of comparison* to the uninsulated and insulated cores placed in the E helix. This method is necessary when the magnetizing effect of the helices are not the same. The uninsulated core which remains in the W helix stands in the place of the "unit-candle" in Bunsen's method of photometry, and the needle which is moved to such a position between the helix that it remains at 0° when the circuit is open or closed is the equivalent of the paper screen with its central translucent circle. But if it should happen that when the needle is placed midway between two similar helices that it remains at 0° when the circuit is open or closed, then two cores would answer the purpose.

Calculation of the relative forces of the uninsulated and insulated cores.—The data for the calculation are given in Experiments 6 and 10.

$$W.UC : E.UC :: 40 \cdot 4^{2 \cdot 7404} : 55 \cdot 6^{2 \cdot 7404} : 1 : 2 \cdot 39925.$$
$$W.UC : E.IC :: 40 \cdot 6^{2 \cdot 7404} : 55 \cdot 4^{2 \cdot 7404} : 1 : 2 \cdot 34374.$$

Hence *Uninsulated Core*: *Insulated Core*:: 1·: 0·9768.

The insulated core is therefore ·02318 weaker than the uninsulated.

In Expts. 8 and 9 it was seen that the above difference of ·02318 in magnetic force caused a deflection of 55′ in the com-

pass needle, and as 3′ or even less of deflection can be accurately read it follows that a difference of $\frac{1}{1000}$th in the magnetic force of the two cores can be detected and measured by this method. This example will convey an idea of the delicacy and accuracy of this plan of measurement, which no doubt can be increased by the careful selection of the proper force of current, distance of helices and other related functions of the apparatus.

It thus appears that this result sides with the theory of Ampère, and also, to my mind, points to that postulate of the atomic hpyothesis which states that the atoms of bodies are separated from each other by distances which are great when compared to the size of these atoms.

Attempts to determine what thickness of tube in fraction of its diameter, is equal to a solid cylinder of the same diameter.—According to the theory of Ampère, the magnetization of a bar of steel, or of an iron core subjected to a helical current of electricity, is due to the surface currents, which are the resultants of the interaction of all the molecular currents in the interior of the bar. It is therefore exceedingly interesting in a theoretic point of view to determine accurately what thickness of tube is equivalent to a solid cylinder of the same length and diameter. The experiments of Barlow, Harris, de Haldat, and of Du Moncel have shown the truth of this deduction.

There is evidently a certain thickness required to develope an equivalent action, but a certain portion of the interior of an electro-magnetic core can be removed without diminishing its force *when in a condition of "saturation."* This determination will therefore inform us, (accepting the hypothetical theory of Ampère), what central currents are prevented by their interaction from affecting the resultant surface currents of the bar; and it appears to me that we should express the result in terms of the central cylinder which can be removed and not of a certain thickness of tube which remains.

Out of well annealed soft iron a series of tubes, 10 in. long, were turned so that they fitted closely into each other; into the innermost tube slid a solid iron core of ·83 in. diameter. The diameter of the outer tube was 1·68 in. With this construction of core, tubes of various thicknesses could be rapidly produced and compared in effect. Two such cores were constructed.

Ex. 12. 400 uninsulated wire-core in W helix; in E helix was placed the combination of tubes and core. Needle brought to such a position that it stood at 0° when circuit was open or closed. T. G. 46°.

Ex. 13. Removed inner core and tubes, and drilled out the innermost remaining tube until the thickness of the combination tube was $\frac{1}{6}$ of its diameter. Current passed. Needle re-

mained at 0°. On further decreasing the thickness the S. end of the needle was deflected toward the W helix. T. G. 45°.

It thus appears that a tube of this size having a thickness of $\frac{1}{6}$ of its diameter is, when magnetized with this strength of current (8 cells, T. G. 46°) equal in effect to a solid cylinder of the same diameter.

Ex. 14. Another tube was constructed having a diameter of 1·3 in. and a thickness of $\frac{1}{8}$ the diam. Into this tube slid a solid core. Placing the uninsulated wire-core of 400 in W. helix and tube with inner core in E. helix, the needle was placed so that it stood at 0° when circuit was open or closed. T. G. 46°.

Ex. 15. Withdrew inner core. Current passed. S. 40′ W. T. G. 46°. Thus a tube of the above diameter, having a thickness of $\frac{1}{8}$ of its diameter is not equal to a similar solid cylinder. This subject will therefore have to be the object of further investigation.

Thinking that in the case where a current did not saturate the above tube it would equal the solid cylinder, the following experiments were made.

Ex. 16. The tube used in (14) was placed with inner core in E. helix, and a similar tube and core in W. helix, and the needle placed so that it stood at 0° when circuit was open or closed. A current from 4 cells was passed. T. G. 32°.

Ex. 17. Solid core removed from tube in helix E. Needle remains at 0°. Hence for this strength of current (T. G. 32°) the tube is equal to an equivalent solid cylinder.

Ex. 18. Other things remaining as in (17) a current from 5 cells, T. G. 37°, was passed through helices. Needle S. 2′ W.

Ex. 19. Current passed from 6 cells. T. G. 40°. Needle S. 15′ W.

Ex. 20. Current passed from 7 cells. T. G. 42°. Needle S. 20′ W.

Ex. 21. Current passed from 8 cells. T. G. 44°. Needle S. 40′ W. It is clearly seen from above experiments that with a current of T. G. 32° the tube is equal to the similar solid cylinder, but on gradually increasing the current up to T. G. 44° that the tube falls more and more below the strength of the cylinder, and the needle is again deflected S. 40′ W. toward the cylinder with that current. Therefore in determining what portion of the interior of a cylinder can be removed so that what remains shall equal the solid cylinder we should see that the tube is "saturated," otherwise the comparison does not solve the problem. Whether this condition was fulfilled in Expt. 13 was not ascertained.

In Daguin's *Traité de Physique*, vol. iii, p. 615, Paris, 1861, we read: "M. Du Moncel finds that it is necessary, in order

that the magnetization [of a tube] may remain the same as a solid cylinder, that the thickness should be at least $\frac{1}{4}$ that of the radius of the cylinder." I have not seen Du Moncel's research on this subject and do not know the method he adopted in comparing the magnetization, but I think that his estimated fraction of $\frac{1}{8}$ of the diameter is too low, as Ex. 13 clearly shows. The cause of this error, I imagine, resides in the fact that he used a current not sufficient to saturate the tube and cylinder, and thus his determination would be explained by Expts. 15 to 21.

This subject is worthy of careful research, and its theoretical bearings on the subject of magnetism are very interesting. My experiments thus far cause me to doubt if any *constant* fraction of the diameter will be found, but that this fraction will depend upon the diameter of the tube; being larger as the tube is smaller.

Experiments on a Tube slit longitudinally.—To determine the influence, if any, of destroying the continuity of the exterior surface of a tube by a longitudinal slit, I obtained a tube of well annealed iron, 1·68 inch in exterior diameter, ·16 in. thick and 10 ins. long, and slit it in its whole length by an opening $\frac{1}{20}$ inch wide. This opening could be closed at pleasure by placing in it a tightly fitting piece of iron 10 in. long.

Ex. 22. The above tube, with the slit closed, was placed in the E. helix and the 400 U. C. in the W. helix, and needle brought to such a position between them that it stood at 0° when circuit was open or closed. Eight cells of Bunsen. T. G. 46°.

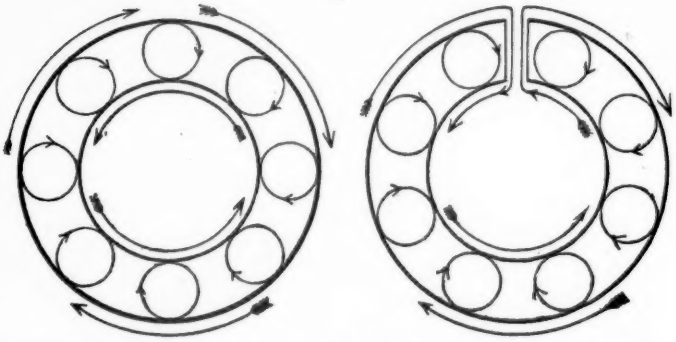
Ex. 23. Slit opened by removing the piece of iron. The needle remains at 0°. T. G. 46°.

Expts. 22 and 23 repeated several times with the same result.

In the accompanying diagrams is shown, according to the theory of Ampère, the direction of the circulation of the exterior and interior surface-currents. It is seen that in both cases the interior surface current circulates in an opposite direction to the exterior current; and that when the tube is slit longitudinally the circulation is not cut off but *facilitated* by the joining of the exterior and interior surfaces. It is therefore natural to suppose that on account of this facility afforded to the circulation that a *quicker* magnetization and demagnetization will occur in the split than in the closed tube; but no difference is found in the magnetic force of the two tubes, for the loss in surface by the slit seems made up in the greater facility of circulation.

I would therefore suggest that the cores of the electro-magnets of astronomical chronographs and of telegraph instruments be of soft well annealed Norway iron, made into tubes having a thickness of $\frac{1}{5}$ of their diameters, and slit longitudinally by a narrow opening.

2.



On the magnetic condition of the interior surface of a tube.—Referring to fig. 2 it will be seen that the Ampèrean currents on the interior surface of a tube flow in a direction contrary to those on the exterior. The magnetic condition of this surface was therefore an extremely interesting point for investigation; it has been the subject of over two hundred experiments, and is by far the most difficult research mentioned in this paper.

In my first experiments, steel rods and iron cores were introduced into soft iron tubes which were then placed in helix E. The current was then passed and the tube with enclosed core very slowly withdrawn. The steel or iron core was then removed from the tube and tested by placing it to the W. of the center of a compass needle. The magnetic force of the core was then compared with what it had before it was introduced into the tube. This method was thoroughly tested by withdrawing tube and core both before and after contact was broken in the helix, but no decisive conclusions could be drawn because the effect of the inductive action which took place when the core was withdrawn from the tube changed the magnetic relations which existed while the core was in the tube.

The following method was then devised. Two tubes of very soft iron were constructed, having both an internal diameter of ·84 inch, and one an external diam. of 1·03 inch, the other of 1·31 inch. Both tubes were 9 in. long. One hundred and twenty well annealed iron-wires a little less than 9 in. long were tightly bound together by silken cord and then wrapped in six thicknesses of writing paper, so that the wire core fitted neatly the interior of the tubes.

The wire core was now deprived as far as possible of magnetism by passing a weak magnet over it, and could thus be so reduced that it caused a deflection of only 1° when placed 7·5 in. W. of the center of a surveyor's compass needle. By now

inverting the core in the line of the dipping-needle this small residual magnetism could be reduced to 10′ or 15′ deflection, and in some experiments even a reversed magnetization was thus produced. The core was now handled very carefully to prevent all vibrations or blows, and placed in an E. and W. horizontal line.

The tube was placed in helix E and the current passed. It was then *very slowly and steadily* withdrawn, so as gradually to let down its magnetism, the tube removed several feet from the helix and the current broken. This operation was repeated until the tube had attained as high a degree of magnetization as could be given to it with the same constant current in the helix, and a further repetition of the process caused no increased deflection in the needle. The tube was then carefully placed in an E. and W. horizontal line and the core slowly introduced, and so that the S. pole of the core was in the S. end of the tube. It is important in all these manipulations to keep the core and tube in a horizontal plane, for if they are inclined to the horizon, during the experiment, the magnetic condition is changed and the operations have to begin anew. The degree of susceptibility of very soft iron to a change of inclination can only be appreciated by those who make the trial.

The tube with enclosed wire-core was now introduced into the helix and the current passed; then withdrawn, and this repeated (as in the above process on tubes alone), until the needle-deflection showed no increase of magnetic force. This deflection was then compared with that previously produced by the tube acting alone.

Now if the interior electric currents really flow as Ampère's theory states, then when the core is in the tube and the maximum magnetization reached, the combined effect of tube and enclosed core on the needle should be less that given by the tube alone without the central core; for, in the former case the core has a magnetism given it the *reverse* of the tube; i. e., supposing marked end of tube S and marked end of introduced core S, then, after magnetization in the helix, the marked end of core is N and will therefore neutralize a portion of the tube's action on the needle. Now this was found to be *invariably* the case in all the experiments made; and how can it otherwise be explained. It is known that the combined force of two magnets with like poles placed together is less than the sum of the forces of the separate bars separately measured; but the force of the two together is always greater than that of one alone. But we here have a tube and enclosed insulated core with like poles together giving, after magnetization (in the same conditions *exactly* as that given to the tube alone) a less effect than that given by the tube alone. Therefore these facts show, to my

mind, that the deduction from Ampère's theory is verified by experiment; and that we have succeeded in giving to a core in the interior of a tube a polarity the reverse of that given to the tube itself.

We here present the following experiments, which are typical of all made; the others only differing in quantity with a change of distance from the needle and a change in the intensity of the current in the helix.

The marked ends of tubes were S. poles, and S. 10° E. means that when tube or core or both were placed W. of the center of the compass-needle that the S. *end of needle* was deflected 10° E. If the same end is deflected W. then the marked end is a N. pole.

Ex. 24. Experiments with tube of 1·03 in. diam. is given under column A; those with tube of 1·31 in. diam. under column B. Tangent galvanometer in all the experiments was at 30°. Tube and core placed $7\frac{1}{2}$ inches W. of needle.

Marked end of tube placed in S. pole of helix and withdrawn (as descibed above). Repeated 6 times	A	B
	S. 17° 18′ E.	S. 21° 40′ E.
Introduced tube again and withdrew	" 17 30 "	" 21 40 "
" " " " "	" 17 30 "	" 21 40 "
Core placed W. of needle	" 0 33 "	" 0 18 "
Core slowly and steadily introduced into tube so that S. end of core was in S. end of tube; then tube and core introduced into helix E. and withdrawn 6 times	" 16 2 "	" 20 18 "
Introduced and withdrew tube and core again	" 16 13 "	" 20 18 "
" " " " " " "	" 16 15 "	" 20 18 "
" " " " " " "	" 16 15 "	" 20 18 "
While tube remained in position W. of compass the core was slowly withdrawn from the tube. Tube now alone deflected	" 17 40 "	" 21 38 "
Core now alone deflected	" 0 50 "	" 0 10 W.

Referring to Expts. A and B it is seen that when the tubes were magnetized alone as fully as can be by the current (of T. G. 30°) through the helix, they deflect the needle in Exp. A 1° 15′ more and in Exp. B 1° 22′ more than when the tubes with enclosed cores acted together on the needle after having been magnetized in exactly the same circumstances; also, that when the tubes, remaining undisturbed in their position, W. of the needle, had the cores withdrawn, the needle was deflected 1° 25′ more in Exp. A and 1° 20′ more in Exp. B than when the tubes with enclosed cores both acted upon the needle.

In the break-circuit experiments the effects are more complicated by reason of the instantaneous and intense extra-current in the helix which causes a sudden inductive action on the tube and core, and thus other actions are introduced which are foreign to the object of the investigation. I have therefore only given examples of the make-circuit experiments, though the others showed even a greater difference between the action of the tube

alone and the tube and core together; this difference amounting in some experiments to 5°.

After the results of the above experiments, it occurred to me to investigate the action of *a helix introduced into the tube*, and to ascertain if a magnetism could be given to the tube the reverse of that which is induced when the helix surrounds it. The former experiments show that the tube can give a reversed magnetism to an enclosed core, and we will now see that a helix can effect a reverse magnetism on a surrounding tube.

The first Expts. were made with opposing helices, one being helix E with the 400 wire core, the other a tube of soft iron 9 in. long, 1·3 in. external diam. and ·12 thick, around which was wrapped a helix of one layer of 148 turns of $\frac{1}{20}$ inch wire, and in its interior fitted a helix also of one layer 148 turns of the same wire wrapped upon a cylinder of wood. This last I will call the inner helix, the other the outer helix.

Ex. 25. The inner helix was placed in the circuit (near the battery, far removed from the compass) and the compass was so placed that its needle stood at 0° when circuit was open or closed. The inner helix was now placed inside the tube and the current passed through helix E and the outside helix; and also through the inside helix in a direction the same as the current in the outer helix; and the needle was noted. The current was now reversed in the inner helix and needle again noted; but as the opposing helices were 8 ft. apart, and the current not strong, no effect (after allowance made for direct action of inner helix on needle) was obtained from the action of the inner current.

Ex. 26. I then devised the following experiment. The opposing helix E with 400 wire-core was removed and the compass-needle brought to 0° by the action of the earth's magnetism. The iron tube was then placed 1 foot W. of center of needle with its S. pole toward the compass. The deflection produced by its magnetism was S. 4° 40′ E. The inner helix was now placed in the position W. of the compass previously occupied by the tube and the current passed so that its S. pole was opposed to the needle; the deflection was S. 3° 13′ E. I now placed the helix in the tube and both 1 ft. W. of needle and passed current so that S. end of helix was in S. end of tubes. The deflection produced was S. 4° 50′ E.

From above it is seen that when tube and inner helix act together on the needle the deflection is only 10′ greater than when the tube acts alone, although the helix acting alone causes a deflection of 3° 13′. The neutralization of such a quantity of magnetism can only be accounted for by the fact that the greater part of the magnetism of the tube was reversed by the action of the heliacal current on the inner surface of the tube.

Ex. 27. To obtain directly a reversed magnetism by the

action of the *outside of the helix* the following experiments were made. Several wires 9 in. long and $\frac{1}{20}$ inch diam. were heated in a cupel furnace to bright redness and slowly cooled; one end of each wire was then marked. A wire was then taken and placed with its marked end 2·75 in. W. of the center of the compass-needle and the deflection produced was S. 18′ E., showing that the marked end was a feeble S. pole. A current was now passed through the inner helix and the wire laid upon it so that its marked or S. end was upon the S. pole of the helix, after remaining here for a few moments it was conveyed to the compass and placed, as before, with its marked end 2·75 in. W. of the center of the needle. The deflection was now S. 28′ W., showing that the magnetism of the wire had been reversed and that a wire placed on the *outside* of a helix has given to it a magnetism the reverse of that given when it is placed *inside*.

Ex. 28. Twelve wires were now placed with their marked ends 3·25 in. W. of the needle, and the deflection being S. 8′ E. showed that their marked ends had a slight S. magnetism. They were now removed and placed with their marked ends on the N. pole of the helix at equal distances from each other around the helix, through which a current was passing; they were then removed from the helix before the current was broken and again placed in their former position 3·25 W. of the compass-needle. The deflection was now S. 55′ E., showing that their marked ends were of south magnetism. They were again placed on the helix with their marked ends on its S. pole and removed, as before; they now deflected the needle S. 20′ W., showing that their marked ends were now N., their magnetism having been reversed.

Ex. 29. The inner helix alone was placed 1 foot W. of needle and the current passed so that its S. pole was opposed to the needle. Deflection S. 3° 30′ E. T. G. 20°. Reversed current. Deflection 3° 15′ W. By holding a bar of soft iron in the line of the dip and passing the wires over its end I succeeded in rendering them without action on the helix when put in the place of the helix in the above experiment. The twelve wires were then tied around the helix equidistant from each other and separated about ·2 inch. The current was then passed so that the S. pole of the helix was opposed to the needle. Deflection S. 2° 40′ E.. Reversed the current. Deflection S. 2° 30′ W. We therefore have in the 1st experiment 50′ less and in the 2d 45′ less action on the needle when the wires are around the helix than when they are away. These results correspond to the above experiments on the tube, the helix giving the wire a polarity the reverse of its own.

Beccaria, Coulomb and Faraday have, by their well known experiments, proved that frictional electricity when *at rest* only exists on or just within the outer surfaces of bodies, and Prof.

Joseph Henry, by many experiments, has shown that this is also true when the same species of electricity, of high tension, is *in motion*. The following beautiful experiment, so conclusive in its evidence, appears so little known, that I will here give it as stated by that philosopher. "A copper wire, of the size usually employed for ringing door bells, passed through the axis of an iron tube, or a piece of gas pipe, about three feet long. The middle of this wire was surrounded with silk, and coiled into a magnetizing spiral, into which a large sewing needle was inserted. The wire was supported in the middle of the tube by passing it through a cork at each end, covered with tin-foil, so as to form a good metallic connection between the copper and the iron. On the outside of the tube and opposite each other were placed two other magnetizing spirals, their ends soldered to the iron. When these two spirals were also furnished with needles, and a discharge from a Leyden jar sent through the apparatus, as if to pass along the wire, the needle inside of the iron tube was found to exhibit no signs of magnetism, while those on the outside presented strong polarity. This result conclusively shows that, notwithstanding the interior copper wire of this compound conductor was composed of a material which offered less resistance to the passage of the charge than the iron of which the outer portion was formed, yet when it arrived at the tin-foil covering of the cork, it diverged to the surface of the tube, and still further diverged into the iron wire forming the outer spirals. We must not conclude, however, from this experiment, that the electricity actually passes on the outside of the tube. On the contrary, we must infer from the following fact, that it passes just within the surface. If the iron be coated with a thin coating of sealing wax, the latter will not be disturbed when a moderate discharge is passed through it, though with a large discharge in proportion to the conducting power of the rod, the outward pressure may become so great as to throw off the stratum of sealing wax."

Barlow and Harris have made experiments which show that magnetism is also a surface action; and in Exp. 11 of this paper we saw that when the surface of a wire-core was diminished by compressing the bundle the magnetism diminished with it. To show that this diminution of force was not, in major part, owing to the increased repulsion produced between the bars when brought nearer together, the following experiments were made.

Ex. 30. About 200 $\frac{1}{36}$ inch wires were pressed together as tightly as could be by binding them in a bundle with silken cord, and the deflection they caused in the needle, when magnetized in the helix, was noted; they were now taken apart and bound as tightly as before around a wooden cylinder about 1

inch in diameter; and being magnetized again in the helix with the same strength of current, the bundle caused a far greater deflection in the needle than when it acted without the central wooden cylinder. I consider this experiment as very conclusive of the surface action of magnetism, for in the two measures we used one and the same mass of metal, subjected to exactly the same magnetizing influence, and only differing in the extent of exterior surfaces existing during the two experiments. That the increase of force with the surface was not owing to a change of distance of the wires from the interior surface of the helix is conclusively shown in the next section.

But there are differences to be made between these analogous phenomena of frictional electricity and of magnetism; in magnetism a *considerable thickness* of metal is required to develope this action at the surface; so that (Ex. 13) a tube must have a thickness of about $\frac{1}{6}$ of its diameter to equal a solid cylinder of the same length and diameter, both being, when compared, "saturated" with magnetism; also, it appears (Ex. 26) that a magnetic action can be effected on the interior surface of a tube, while no similar action can be obtained with frictional electricity.

Experiments to determine whether a change of positon of a bar in the interior of a helix causes a change in the intensity of its magnetization.—Theory indicates that no change in the degree of magnetization will follow a change of position of a bar in the interior of a helix, and the following experiments conclusively prove the truth of this deduction.

Ex. 31. In the interior of helix E resting on the bottom of the opening was placed a cylinder of soft iron ·83 inch diam. and 9 ins. long. Opposed to this helix was helix W, (in the same circuit) with the 400 wire-core. Needle was brought to such a position that it stood at 0° when the circuit was open or closed. T. G. $44\frac{1}{2}°$.

Ex. 32. The axis of the iron cylinder was now made to coincide with the axis of the helix. Current passed. Needle still at 0°. T. G. $44\frac{1}{2}°$. Thus showing that the change of position makes no difference in the intensity of the magnetization.

Experiments on the comparative magnetizing effects of a helix and of a combination of spirals, formed of similar wire, and containing the same number of turns, arranged in a length equal to that of the helix.—According to the theory of Ampère the currents which encircle a magnetized iron bar or a steel magnet are in planes at right angles to the axis of the bars; and it seemed to me interesting to determine with this sensitive apparatus what difference, if any, existed between the magnetizing effects of a helix whose turns were inclined in the successive layers alternately in opposite and equal angles with the axis,

and the effects of a combination of spirals composed of an equal number of turns of wire as the helix and existing in the same length.

Fifty spirals, each composed of 14 ft. 8·06 ins. of $\frac{1}{72}$ inch wire in 20 turns, were made by a process to be described in a subsequent communication. These spirals were placed vertically in a frame at equal distances from each other so that they formed a cylinder 9 ins. in length, 3·9 ins. in exterior diameter and with a cylindrical axial opening of 1·68 ins. The turns of the spirals were carefully insulated from each other by saturating the covering of the wires with melted parafine.

A helix was constructed of the same wire, wrapped in 20 layers, each layer consisting of 50 turns. The wire in each layer was wrapped parallel with two lengths of twine so that the 50 turns in a layer occupied a length of exactly 9 ins. This gave a "pitch" to the turns of the helix of ·18 ins. and the innermost turns of the helix formed an angle of 1° 57′·25, and the outside turns formed an angle of 50′·5 with the axis of the helix, alternately to the right and to the left, as it was wrapped. Each layer of turns of wire and twine was carefully saturated with melted parafine of a high temperature, so that the copper was seen through the saturated covering after the parafine had solidified.

Ex. 33. The helix and combination of spirals were placed 8 feet apart and an uninsulated 400 wire-core placed in each. The compass was placed midway between them and the needle brought to 0°. The current was now passed so that the N. pole of the cores faced the compass. The S. end of the needle was slightly deflected toward the helix, showing that the core of this was somewhat stronger than that of the spirals.

The greater strength of the helix could not be attributed to the excess of wire it contained over the spirals, for this only amounted to about 2 inches; thinking that the intense inductive action of the spirals on each other might have some influence, the following experiments were made.

Ex. 34. Placed the needle between helix and spirals so that it stood at 0° when the circuit was open or closed. Then I introduced between the spirals 49 copper discs having central openings a little smaller than those of the spirals. This arrangement, as Prof. Joseph Henry has shown, so effectually cut of the mutual inductive action of the spirals that, on passing a current through them and breaking a mercury contact, the spark of the "extra-current" was (on account of the greater resistance of the spirals) less than when only the circuit of the battery wires was similarly broken. With the exception of the interposed copper discs things remained as in Exp. 33. Current passed. Needle remained at 0°. Showing that the inductive

action had no influence on the intensity of the magnetizing effect. I am therefore of the opinion that the increased effect of the helix was due to superior insulation.

Ex. 31. The combination of 50 spirals was separated into two, each containing 25 spirals. Between the spirals of one combination were introduced the 25 spirals of the other, so that every alternate spiral belonged to the same combination. If the current is now passed through one of the combinations and the two terminal wires of the other combination joined, Faraday has shown, that the "extra-current" in the first combination is entirely given up to second. The spirals thus arranged, with the terminals of the interposed spirals separated, were placed opposite the helix E, and distant 8 feet, and the needle so placed between them that it stood at 0° when the circuit was open or closed. On connecting the terminals of the interposed spirals and passing the current, the needle remained at 0°. Thus conclusively showing that *in a combination of spirals or in a helix the inductive action of the wire on itself or of adjoining spirals or turns on each other has no effect on the power of their magnetization and therefore no effect on the intensity of the current passing through them.*

I do not remember ever having seen a solution of this question, and these experiments have given it under conditions of a very strong inductive action, and with a very delicate apparatus for detecting any effect which might have been produced. The result is one which has an important theoretic bearing on dynamical inductive action, but I reserve for another communication my views on that point.

In bringing this research to a conclusion I think I may safely say that these results and experiments have shown the delicacy and precision of this method of comparing and measuring the electro-magnetic forces; and at a future time I propose using it to solve the problems which relate to the variation of the intensities of cores with their diameters and with their surfaces, and to examine the varying magnetizing effects of helices of different lengths, diameters, and number of turns of wire, and traversed by currents of various intensities.

Some of the above experiments, which relate to the inverse polarity given to cores placed inside of magnetized tubes and wires placed outside of helices, are very suggestive as to an explanation of diamagnetism, but I reserve for the present the hypothetical notions which they have originated in reference to those phenomena.

South Bethlehem, Pa., June 2, 1870.

ART. XXI.—*Abstract of the Second Series of Professor Meissner's Researches upon Electrized Oxygen;** by GEORGE F. BARKER.

IN the first series of Dr. Meissner's researches upon oxygen, published in 1863—an abstract of which, by Professor Johnson, appeared in this Journal, volume xxxvii, page 325, and volume xxxviii, page 18—he arrived at the remarkable conclusion, that oxygen, under the influence of electrical tension, was converted not only into ozone, but also into another modification which always appeared simultaneously, and which formed, when brought into contact with watery vapor, especially after the absorption of the ozone, a peculiar dense mist. This second modification of oxygen Meissner identified with Schönbein's antozone. So remarkable were these results, and so important their bearing, if true, not only upon our theories of ozone itself, but also upon the philosophy of chemistry, that Meissner desired to repeat his experiments, studying particularly the character of the antozone-mist and the effects of electrical tension upon the volume of the oxygen submitted to the discharge. The results of these experiments constitute the paper now referred to.

I.

Section I is devoted to "Electrized Oxygen." The experiment which is to be critically investigated is thus described: "Oxygen is submitted to the action of electricity in a Siemens's or von Babo's apparatus, is then passed into a receiver containing a concentrated solution of potassium iodid, in which the ozone is completely absorbed, and finally through water contained in a second receiver; the gas as it issues from the water, forms above it a thick white mist, which also appears in a less degree over the solution of potassium iodid, but which is denser, the less concentrated the solution and the more favorable the ozonizing conditions." To prove that this mist consists solely of electrized oxygen and water, Meissner proposes to show:—1st, that no other gas but oxygen is in any way concerned in the production of the phenomenon; particularly no nitrogen, chlorine, hydrogen, or carbonic acid. 2d, that for the production of the result, the presence of aqueous vapor in the electrizing tube is not necessary. And 3d, that the potassium iodid solution used for the absorption of the ozone, has nothing whatever to do with the appearance of the phenomenon, further than is implied in effecting the removal of the ozone from the current of electrized oxygen.

* Neue Untersuchungen über den elektrisirten Sauerstoff. Von Dr. G. Meissner. Mit zwei lithographirten Tafeln. Aus dem vierzehnten Bande der Abhandlungen der Königlichen Gesellschaft der Wissenschaften zu Göttingen. 4to, pp. 110. Göttingen, 1869.

The apparatus used to establish the first point, consists of two pieces, the first of which is intended for the production of chemically pure oxygen, the second for the electrization of this oxygen, and its subsequent analysis. We have space here for only a general description of these marvellously accurate specimens of Geissler's glass-work, referring those specially interested to the original plates. The oxygen is evolved by electrolysis from acidulated water contained in a U tube, and then passes through a second U tube filled with bits of glass moistened with sulphuric acid, then through a straight tube filled with oxydized copper-turnings to which heat can be applied, then through a second U tube containing glass and sulphuric acid, and finally through a horizontal tube a foot in length, containing anhydrous phosphoric acid: being collected, after passing an ingeniously-constructed mercury-valve, in a receiver over mercury. All these tubes are joined, either by fusion or by the most carefully ground joints. By means of the heated copper oxyd, the hydrogen, which, by diffusion or mechanically, may be mixed with the oxygen,—as well as the ozone produced by the electrolysis, and the vapor of hydrogen peroxyd—if any there be—are destroyed. The entire apparatus is first washed out with several liters of pure oxygen chemically prepared, then with 15 liters of the electrolytic oxygen; after which the oxygen, being assumed pure, is collected. The second piece of apparatus was made in duplicate; one has a tube filled with sulphuric acid between the receiver and the electrizing tube; the other has a tube filled with anhydrous phosphoric acid in this position. Moreover, in the former, Siemens's tube, in the latter von Babo's, is used for the electrization. With these exceptions they are alike; a description of one suffices. The gas passes from the receiver through the tube filled with the anhydrous phosphoric acid, to free it from any traces of moisture it may have acquired in the receiver, then into the electrizing tube, thence into two small flask-shaped receivers, the first containing potassium-iodid solution, the other water—and then to the Jolly's mercury pump. Between the drying tube and the electrizing tube is a barometer tube by which the exhaustion may be regulated. After thoroughly washing out the entire apparatus with pure oxygen, it is thoroughly exhausted, the cock beyond the electrizing tube closed, and the one connected with the receiver gradually opened, the electrizing tube thus filled with oxygen, and this submitted to the action of the silent electric discharge.* The electrized oxygen now passes successively through the solution of potassium iodid and the water, upon the surface of which latter liquid the dense anto-

* The electrizing tube of von Babo is described in Prof. Johnson's abstract above alluded to.

zone-mist appears, increasing momentarily in density until the flask, two inches in diameter, becomes perfectly opake. This experiment, performed in precisely this way, was repeated twelve times, and always with the same result; Dr. Meissner believes therefore, that neither hydrogen nor nitrogen is in any way concerned in the production of this peculiar mist.

The second point, that the presence of aqueous vapor in the electrizing tube is not necessary to the result, was established by filling this tube with pure oxygen dried over anhydrous phosphoric acid, then electrizing it, and, after intermitting the current, passing it through the solutions. The result was the same as before.

Nitrogen having been shown to be without effect on the production of the mist, the especial apparatus for the electrolytic preparation of oxygen was given up, and this gas was obtained from potassium chlorate, and washed with a solution of potassium iodid. In the next experiment this oxygen passed from the gasometer through a wash-bottle filled with potassium hydrate solution, then through a U tube filled with calcium chlorid, then through another wash-bottle containing sulphuric acid, and finally over anhydrous phosphoric acid, to the electrizing tube, being now free from chlorine, carbonic acid, ammonia, and moisture, though containing perhaps a trace of nitrogen. On passing a stream of this electrized oxygen, first over phosphoric anhydrid, then into iodid of potassium solution, and afterward through water, the dense cloud appeared as before. Meissner believes therefore "that these experiments, not once only or a few times repeated, but performed very frequently, prove the following point: that the mist formed by de-ozonized electrized oxygen with aqueous vapor, appears when neither chlorine, nitrogen, ammonia, hydrogen, carbonic acid nor watery vapor is present in the tube where the electrizing occurs; and that the presence or aid of neither of these substances is necessary afterward for its formation. That in other words, the mist-phenomenon requires only dry electrized oxygen, the potassium iodid used for de-ozonization, and the vapor of water, for its production."

The third point, that the potassium iodid acts simply by absorbing the ozone, and thus setting the mist-forming oxygen free, Meissner proves by replacing this substance by a great variety of other bodies, differing widely in chemical properties and agreeing only in the property of absorbing ozone. Sodium pyrogallate,—which must be free from even a trace of gallic acid—potassium ferrocyanid, potassium manganate, the higher sulphids of potassium and sodium, barium sulphid—the hydrosulphuric gas being removed by passing the de-ozonized oxygen through a solution of cupric sulphate before entering the re-

ceiver of water—ammonium sulphid, hydrosulphuric acid, sulphurous acid—either free or as sodium sulphite—and ammonio-ferrous tartrate, were all used with good results, the ozone being entirely removed. It is not necessary however, to absolutely de-ozonize the oxygen, in order to produce the mist; partial absorption will produce it, though it is weaker in proportion to the amount of ozone remaining. By using potassio- or sodio-ferrous tartrate, instead of the ammonia salt, such a partial absorption is effected. The same is true of certain metals, when moistened with water; lead, zinc, copper, iron, tin, cadmium, antimony, aluminum, and thallium all giving the mist, though lead, zinc and aluminum are the best for this purpose. Meissner compares the mist rising from these metallic bars to the cloud which rises from moistened phosphorus. Even carbon, in the form of gas-carbon, and sometimes animal charcoal, effects this absorption. The noble metals gold and platinum remain completely unaltered in both moist and dry ozonized oxygen. Chemically pure silver in the dry gas remains unchanged except at single points which show a beautiful greenish play of colors, without a trace of black peroxyd, into which, in the presence of moisture, it is so rapidly converted. Mercury, perfectly dry and still, remains unaltered, though on the least agitation a film appears on its surface, by which it adheres strongly to the glass. On replacing the ozonized by common oxygen, the mercury becomes again mobile, a black powder—of mercurous oxyd probably—being left. If the mercury be moist, reddish mercuric oxyd results. With none of these latter metals, however, is any mist formed.

Though these experiments prove conclusively that no single substance can be yielded by such widely different de-ozonizing substances, to form the mist, yet may not each yield one, which, though unlike the others in all else, may have, in common with them, this mist-forming property? This hypothesis, Meissner considers disproved by the fact that the mist is chemically identical, whatever the absorbing agent. It may be agitated with, or passed through water, not only without losing, but often with actual increase of, its properties. It may be passed through dilute sulphuric acid or alkaline solutions unchanged. "In a word, the mist which appears in my experiments under the given conditions, is neither acid nor alkaline, consists of a body neither soluble nor insoluble in water, but is solely a mechanical or adhesive combination of oxygen and water, which, when washed and collected in a gas-holder, gradually disappears, fine fluid drops collecting upon the walls of the vessel which when examined are found to be pure water, containing possibly under certain conditions, a trace of hydrogen peroxyd." When passed through strong sulphuric acid, or over

calcium chlorid or phosphoric anhydrid, the cloud disappears because of the removal of the water; but it reappears again, though weaker, when brought again in contact with water. The unwashed mist, indeed, contains impurities coming from the absorbent employed; as iodine and traces of iodic acid from the potassium iodid, ammonia, hydrosulphuric gas, and probably, also, traces of ammonium nitrate. But all de-ozonizing substances do not yield such impurities, and even these may be removed without affecting the mist.

Again, may not the mist be produced either by the action of the oxygen unaffected by the electricity, or by the action of the ozone which thus results? Oxygen itself, under no known conditions, exhibits this property; and as to ozone, the above experiments show that it may be completely removed by various absorbing solutions without interfering with the phenomenon. Indeed, the mist is the stronger, the more perfect the removal of the ozone from the electrized oxygen. So that, contrary to the opinion expressed in the first paper, not a trace of mist appears until the ozone has been, at least partially, removed. That this mist is not due to a compound of ozone with water, is established by the fact that contact of the electrized oxygen with water before de-ozonization is prejudicial to its subsequent production. Further, such a compound must hold the ozone with such force as to prevent such easily oxydized bodies as potassium iodid, pyrogallic acid, or alkaline sulphids, from withdrawing it, and yet be at the same time one of the most unstable of substances. And moreover, were there such a compound of ozone and water, the de-ozonized oxygen, after careful drying, would yield water when subjected to a high temperature; Meissner however, has failed entirely to detect any moisture under these circumstances. Finally, the only known compound of water and oxygen, hydrogen peroxyd, forms no such mist. If therefore, neither the ozone itself, nor any compound of it, is concerned in the phenomenon, there must be contained in the pure dry electrized oxygen, besides the unaltered oxygen and the ozone, some third body, a third condition or modification of oxygen, to which the result is to be ascribed.

Assuming now the existence of antozone, Meissner proceeds to study more minutely the action of absorbing agents, with special reference to this substance. He divides them into two classes, one of which removes ozone alone from the solution, the other absorbs both modifications. The first substance examined is pyrogallic acid itself, by which the ozone is completely removed, the solution becoming beautiful hyacinth-red in color; but, unlike the action of the alkaline pyrogallates, the free acid removes also the antozone, so that not a trace of mist

appears when the gas is subsequently passed through water. On adding a few drops of sodium hydrate, the liquid becomes dark brown, and the mist at once appears. Meissner accounts for this result, by supposing that in the oxydation of the free acid both ozone and antozone are taken into combination in equal proportions, while in that of its alkaline salts, the ozone combines to a larger extent than the antozone. Another substance examined is sodium thio-sulphite (hypo-sulphite). When the electrized oxygen passes through a concentrated solution of this salt, both ozone and antozone disappear, not a trace of mist being formed. This result is especially noticeable, inasmuch as sodium sulphite removes only the ozone, and inasmuch as the product is, in both cases, sulphuric acid. Since to oxydize sulphurous acid, two equivalents of oxygen are required for every two of sulphur; and to oxydize thio-sulphurous acid four equivalents of oxygen are needed to two of sulphur; Meissner believes that the two of oxygen in the former case are ozone; while the two times two in the latter are not of equal value, one pair being ozone, the other antozone, atoms. So also by the use of arsenous acid as an absorbent, both modifications are removed from the electrized stream of oxygen, both when the acid is free as well as when its sodium salt is employed. By using a very dilute solution, or the ordinary saturated solution in small amount, the ozone may be imperfectly removed; and by passing the gas afterward through potassium iodid solution to remove the ozone entirely, the antozone which remains gives a feeble mist with water; thus showing here also, that ozone and antozone are absorbed by the arsenous acid in equivalent proportions. Here again Meissner believes that of the two atoms needed to make arsenic acid from arsenous, one is ozone, the other antozone. Mercurous nitrate, in concentrated solution, absorbs, under the conditions of the experiment, both ozone and antozone only partially, though equally; thus acting like arsenous acid. The partial absorption in these cases is to be distinguished from that effected by the metals or alkaline ferrous salts; in the former case both the oxygen modifications are equally absorbed, while in the latter the ozone is almost entirely removed, but the antozone is unaffected.

Of particular interest is the action of electrized oxygen upon potassium hydrate. When fragments of this substance slightly moist upon their surfaces are placed in a horizontal glass tube, and subjected to the current of electrized oxygen, they shortly become covered for a short distance from the end of the tube, with orange-yellow potassium peroxyd. But however long the experiment continues, the other pieces remain unchanged, and not a trace of either ozone or antozone issues from the tube. So soon as the electrical action ceases, and ordinary oxygen

again enters the tube, the yellow crust disappears, to be again formed as often as the electrized oxygen passes over it. Since the peroxyd is at once decomposed by water, and since too, the maximum production is very soon reached, it would seem that the peroxyd is successively produced and decomposed in the experiment, causing in this way the disappearance of ozone and antozone and yielding common oxygen. Absolutely dry hydrate has no effect on the electrized oxygen. In aqueous solution, the formation of the yellow peroxyd takes place only in the narrow tube which delivers the gas; but as the antozone and the ozone are materially lessened, though never absolutely destroyed in any of Meissner's experiments—it is fair to infer an action similar to that in the tube. Sodium hydrate acts similarly though less energetically; the two modifications appear moreover to be unequally absorbed, more of the antozone disappearing than of the ozone. These results illustrate well the two classes of absorbing agents; in the one case complete absorption of the antozone takes place with that of the ozone, in the other a portion of the antozone remains. In the one case partial absorption of both is equal, in the other unequal. One of these classes requires for its oxydation more ozone than antozone, the other requires them in equal proportions; possibly there is a third class requiring more antozone than ozone.

An important practical application is made of these facts to the employment of potassium iodid as an absorbing solution. A neutral solution of this substance becomes alkaline when subjected to the action of ozone, and contains free potassium hydrate, which, acting as above, diminishes the antozone. By so arranging the apparatus that acid or alkali can be added at pleasure to the iodid solution, the mist is seen to be diminished on making it alkaline and increased when it is made acid. The neutral solution of potassium iodid is not therefore a suitable de-ozonizing agent, if it is desirable subsequently to produce the mist. It should be previously acidulated with hydrochloric or sulphuric acid, especially where the quantity of antozone is small. Though for very delicate investigations Meissner prefers an aqueous solution of iodine.

The same injurious action of free alkali appears with ferrous oxyd and pyrogallic acid solution. It is completely obviated by using ammonia in place of potassa or soda, ammonium pyrogallate and ammonio-ferrous tartrate being far preferable to the same compounds of the fixed alkalies. Even with the sulphids this is true; the strongest antozone-mist Meissner observed was obtained by using ammonium sulphid as de-ozonizer, this compound absorbing more ozone and less antozone than any other substance tried. When ammonium hydrate is submitted

to a stream of electrized oxygen, the ozone is partially absorbed by it, and the antozone cloud rests on its surface. By passing the mist through dilute sulphuric acid, its ammonia may be removed; and then, on collecting it in an open vessel, the water which it deposits is found to contain nitric acid which comes *from the oxydation of the nitrogen of the air.* Solutions of ammonium carbonate act in the same way. The energy with which ammonium sulphids absorb ozone, is further shown by the fact that, like moist phosphorus, some of them can polarize common oxygen, absorbing the ozone and emitting a powerful antozone-mist. Under the name "Liquor fumans Boylii," such an ammonium sulphid has long been known. It fumes strongly, but only with oxygen; neither hydrogen nor nitrogen causing it. Every unsaturated ammonium sulphid solution, if it contain sufficient sulphur, is such a fuming liquor. By passing pure oxygen through it and then through water, a dense mist is obtained which can be washed, be dried, and then be reproduced; a true antozone mist.

A singular cause of error in his previous paper is here pointed out. When dry air is electrized, the mist is easily obtained on contact with water, even without the previous use of any de-ozonizing solution. Meissner hence stated that the mist could be formed without previous removal of the ozone. But in fact the nitrogen of the air, being oxydized to nitric acid by the ozone, acts as a de-ozonizing agent. He obtained however the same result with pure oxygen. But upon examining his apparatus, he found a cork previously used with a solution of potassium iodid, which had iodine upon it and thus acted as the de-ozonizer. In his later researches all these sources of error were avoided, and the conclusion established that to produce the antozone mist, the ozone must first be wholly or partially removed.

"As the result of many oft-repeated experiments," says Meissner, "it may be asserted that there is no single fluid through which, or no single solid over which, electrized oxygen can be passed without exerting some action upon it; even when the substance is itself not oxydable, or does not fix in chemical combination any constituent of the electrized oxygen, some action takes place by which the amount of ozone or antozone in the stream of oxygen is more or less diminished." Not only finely divided gold and platinum, but also entirely indifferent bodies, such as asbestus, cotton, calcium chlorid, charcoal-powder, or finely broken coal, destroy the ozone and antozone in electrized oxygen. Even after the removal of the ozone, if the mist be passed over finely granular calcium chlorid, the antozone will disappear. The same is true of liquids; a sulphuric acid valve included in an apparatus to

prevent the entrance of moisture into the electrizing tube, lessens the amount of ozone and antozone obtained.

II.

Section II treats of the "Quantitative estimation of Ozone, and the contraction of volume on electrizing oxygen." The apparatus employed is similar to that above described, the electrizing tube being widened to enclose a thermometer, and having a delicate mercury-manometer attached to it. A bulb-apparatus of simple construction holds the de-ozonizing solution, and a second similar one contains sulphuric acid. The last bulb of the former apparatus contains fine asbestus, which is found to destroy almost perfectly the cloud-forming substance, and so to retain the iodic acid and iodine carried over with it. Both bulb-tubes have wires by which they may be attached to the balance. In making an observation, the apparatus is filled with pure oxygen, the manometer, barometer, and thermometer carefully noted, the oxygen electrized, time allowed for the temperature to be equalized, communication established with the manometer, the diminution of volume ascertained, and the quantity of active oxygen determined: 1st, by the increase in weight of the bulb apparatus, and 2d, by titration with sodium thio-sulphite or occasionally, sodium sulphite. The free iodine is first determined, and then the solution is acidulated with hydrochloric acid; the hydriodic acid thus set free reacts with the iodic acid to set free more iodine, which is then titrated by itself. Hence every iodine-equivalent represents one of oxygen, and by dividing the weight of iodine found by 15·875 (127÷8) the quantity of oxygen absorbed is ascertained; or in other words, the oxygen absorbed shown by the increase of weight, must be to the iodine set free as measured by titration, as 1 : 15·875. In the experimental results given in the table, this proportion is not reached, but varies from 1 : 14·9 to 1 : 13. This variation, observed by other experimenters, was investigated. It was found not to be due to the action of the unozonized oxygen, nor to any loss of iodine in the gas-current, nor to imperfections in the titrition itself; nor did it appear when the iodine was completely oxydized to iodic acid, both weighing and titrating then giving the same results. These last experiments, Meissner believes in passing, establish the fact that ozone is simply a modification of oxygen, not an oxydized water; that no hydrogen peroxyd is produced in it; and that the method of experimenting here employed is free from errors. He believes too, that the variations observed when the potassium iodid is incompletely oxydized, may be due to the production of some intermediate oxyd of iodine; and hence, regards the method by weight as less liable to error in estimating the amount of ozone, than the

method of titration. If, however, the solution of potassium iodid be either very dilute or very concentrated, or particularly, if it be acidulated; or, what is the same thing, if a solution of hydriodic acid be used,—in which case no iodic acid is formed —then the method of titration becomes accurate, the results agreeing with those obtained by weight. The objections however to the acidulated potassium iodid solution are: 1st, there is a loss of iodine, carried off by the antozone; and 2d, the iodine separates in thick masses of crystals which stop entirely the delivery tube. When too, the ozone enters the absorbing solution very dense, as when a given portion of oxygen is electrized for a long time, and then passed at once into it, the differences between the results of weighing and titrating are very much increased; the results then varying from 1:8 to 1:4, instead of 1:15·8.

The experiments on the influence of electrical tension upon the production of ozone showed that with a velocity of 2·25 to 2·50 liters per hour, a spark of 1·5 centimeters gave ·006 grms. ozone, of 4 to 5 centimeters ·0274 grms, of 5 to 6, ·0322 grms., and of 6 to 7 centimeters ·0389 grams, as the maximum; no increase being obtained with that apparatus, on increasing the length of the spark.

On the question of the contraction of volume on electrizing oxygen, a series of twelve experiments, made with the apparatus already described, show that, as a mean, the weight of oxygen absorbed as ozone, bears to a weight of oxygen equal to the observed contraction, the ratio of 1·984 to 1; i. e., essentially of 2:1. That is to say, the portion of electrized oxygen absorbed by potassium iodid, weighs twice as much as the volume of oxygen which disappears during the electrization. This result was so extraordinary, that the experiment was carefully repeated, the potassium iodid being completely oxydized; the oxygen absorbed weighed 0·0182 grams, the contraction of volume 0·00938 grams, being virtually 2:1. Meissner is not prepared to claim this numerical relation, however; he simply maintains that the electrized oxygen absorbed by the iodid, weighs more than the volume corresponding to the contraction observed. He then goes on to discuss the methods by which Andrews and Tait, and von Babo and Claus, came to different results; criticising in the former case, the apparatus used, the formula for calculation, and the method of determining the ozone by titration; and in the latter, the use of a neutral solution of potassium iodid having a wrong concentration, and the velocity of the stream of oxygen, both of which, as shown above, affect the result. As to Soret's results, which were made with electrolytic oxygen, Meissner merely remarks that if made with the use of a neutral solution of iodid as absorbing agent, as seems to be the case, they may not be reliable; though not

having prepared ozone in this way, he can pass no further judgment upon them. No conclusion however, upon the density of ozone can be drawn by Meissner from his own experiments, since the results obtained are due to both the modifications of oxygen produced by the electrization; while the ratio of the two present, is unknown.

Finally, Meissner takes up the question of the electrizing of confined oxygen. He finds that the limit of the production of ozone is nearly reached in the first 10 to 20 minutes; that on diminishing the electrical tension after a time, the amount of ozone is increased; that the maximum contraction is 8·61 per cent, corresponding to $\frac{1}{11\cdot6}$, instead of $\frac{1}{12}$ as obtained by Andrews and Tait; that more than eight times the quantity of ozone is produced when the oxygen flows in a current through the electrizing tubes, than when confined, with an equal tension; that the agency which causes the return of the ozone and antozone to common oxygen, increases in activity with the density, with the percentage of electrized oxygen, and, in a given apparatus, with the electric tension; so that when this electrized oxygen reaches a certain density, the further action of the electricity destroys as much ozone and antozone as is produced. He believes that the electrization of the oxygen is dependent on the tension, the destruction of the ozone and antozone on the character of the discharge, so that for every apparatus there is a limit to the production of ozone, beyond which, if the tension be increased and the glass be not broken, the ozone and antozone rapidly disappear. If the oxygen be dry, it retains its diminished volume for many days; but if moist, much less electrized oxygen is produced, and it disappears entirely in the course of two days. Mercury appears also to have a specific action in destroying electrized oxygen.

This second research of Dr. Meissner, though not as startling in its results as the first, is yet quite as valuable a contribution to science. He has fully established the main positions taken in 1863, and it cannot longer be doubted that electrized oxygen contains two modifications of this element. The bearing of this fact upon our theories of ozone is evident.

ART. XXII.—*Description of Sclerostoma pinguicola, a new species of Entozoa, from the Hog;** by A. E. VERRILL.

ON two occasions I have received specimens of a rather large parasitic worm, which lives in the fat of hogs. In the first case, five specimens were obtained, at New Haven, by Dr. M. C. White, from the fatty portion of a spare-rib; in the

* Extracted from "The External and Internal Parasites of Man and Domestic Animals;" by A. E. Verrill, page 138, figure 89*a*, July, 1870.

second instance, at Middletown, Conn., Dr. N. Cressy found large numbers of the worms in the fat about the kidneys of a young Suffolk pig, brought from New Jersey. Unfortunately, none of these specimens are in so good a state of preservation as to enable me to determine with certainty all the points of their structure. Those which I owe to the kindness of Dr. White had been mounted in glycerine, as microscopic objects, and pressed out flat, before they came into my possession, and the tissues were thus injured and the organs deranged. Those from Dr. Cressy were both pressed flat and dried. Yet by careful maceration, and considerable labor, I believe that most of the important characters have been made out. The body is rather robust, especially in the female, and tapers to both ends; the color is yellowish white, and the integument is seen to be finely striated transversely, when considerably magnified. The head is smaller than the body, truncated at the end; the mouth is terminal, roundish, or somewhat angular, surrounded by the thickened rim of the chitinous capsule, or pharynx. The edge of this chitinous ring rises at intervals into four to six denticles, or very small angles, which correspond to thickened, longitudinal, chitinous bands, that strengthen the pharynx, and give a slightly angular form to the mouth. The pharynx itself is small, short, and rather squarish, when seen in profile, and has three or four small, conical teeth at the bottom. The œsophagus is thick, club-shaped, and very muscular.

The male is 1·12 of an inch long, and about ·05 of an inch in diameter; the tail ends in a small blunt lobe, united with two small, entire, membranous expansions, one on each side, forming a small bursa, which is strengthened by several short rays, the exact number of which could not be determined in my specimens; there are two long, slender spicules. The females are 1·25 to two inches in length, and, as flattened between glass, they are ·10 to ·13 of an inch in breadth. The posterior end suddenly and obliquely narrows to a small conical point, which is turned to one side. The anal opening is close to the end, and the genital orifice appears to be adjacent to it. The oviducts are long, voluminous, much convoluted, and unite in a large and capacious uterus, which fills most of the cavity of the body toward the posterior end. The uterus and oviducts are both filled, in the larger specimens, with immense numbers of small oval eggs.

It is probable that this parasite is by no means uncommon.

Figure 1.—*Sclerostoma pinguicola* Verrill; *a*, male, natural size; *b*, posterior end, enlarged, showing the bursa and spicules; *c*, female, natural size; *d*, anterior end of the same, enlarged, showing the chitinous capsule and œsophagus.

ART. XXIII.—*Notes on the structure of the Crinoidea, Cystidea and Blastoidea;* by E. BILLINGS, F.G.S., Paleontologist of the Geological Survey of Canada.

(Concluded from this Journal, II, vol. xlix, p. 58).

6. *On some points relating to the Structure of Pentremites.*

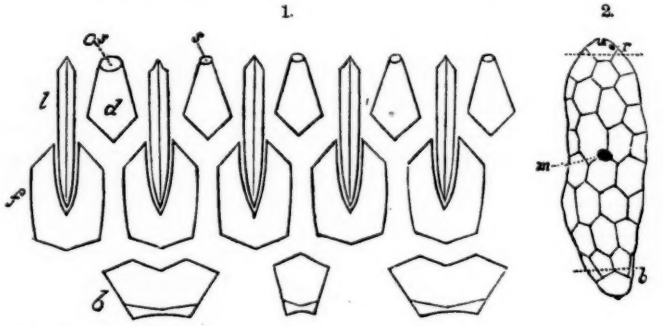


Fig. 1.—Calycine plates of *Pentremites*,—*b*, the basals; *f*, one of the five forked plates; *d*, deltoid plate; *l*, lancet plate; *os*, oral spiracle; *s*, spiracle.

Fig. 2.—*Caryocystites testudinarius*, Hisinger,—*b*, basal plates; *r*, radials; *m*, mouth.

Professor Wyville Thompson has proposed a division of the skeleton of the existing Crinoid, *Antedon rosaceus*, into two systems of plates, which he terms respectively the "*Radial*," and the "*Perisomatic*" systems.* These he considers to be thoroughly distinct from each other in their structure and mode of growth. The radial system consists of the joints of the stem, the centrodorsal plate, the radial plates, the joints of the arms, and also those of the pinnules. In the perisomatic system he includes the basal and oral plates, the anal plate, the interradial plates, and any other plates or spicula which may be developed in the perisom of the cup or disc. This I think a good arrangement, except in so far as it regards the stem, which appears to me to be, always, an appendage of the perisomatic, rather than of the radial system.

Throughout the whole range of the Crinoidea, the plates of the radial and perisomatic systems, are easily distinguished from each other. In general, the Cystidea have no radial plates in their calyces except, perhaps, in a small area around the ambulacral orifice. This accords well with an important observation

* On the Embryogeny of Antedon rosaceus Linck (*Comatula rosacea* of Lamarck). By Professor WYVILLE THOMPSON, LL.D., &c. Philosophical Transactions of the Royal Society, vol. clv, Part II, p. 540.

of Professor Thomson's on the structure of Antedon, while in the earlier periods of its growth. "The entire body of the Pentacrinoid is," he says, "at first, while yet included within the pseudembryo and during its earliest fixed stage, surrounded and enclosed by plates of the perisomatic system alone, and it is quite conceivable that plates belonging to this system may expand and multiply so as to form a tessellated external skeleton to the mature animal, the radial system being entirely absent, or represented only in the most rudimentary form." (Op. cit., p. 541). Such is the structure of all of the Cystidea. On referring to fig. 2, it will be seen that the whole of the body of *Caryocystites testudinarius*, is covered with polygonal plates, without any trace whatever of a radiated arrangement. The plates are disposed in nine transverse ranges, girding the body like so many rings. This species is, (and so are most of the elongated subcylindrical Cystideans), annulated rather than radiated, so far as regards the external integument. The lower range, below the line, *b*, consists of the basals, whilst the upper, above the line, *r*, may, possibly, be radiated. In all the globular or ovate Cystideans, with numerous plates, such as *Spœnonites*, *Malocystites*, *Comarocystites*, *Amygdalocystites*, and others, the shell is neither annulated nor radiated, but composed of an indefinite number of plates, increasing with the age of the individual, and arranged without any well defined or constant order. It seems clear, therefore, that the test of the Cystidea belongs mostly to the perisomatic system.

In *Pentremites* the three plates which are usually called the basals, consist each of two pieces, one placed above the other, and, in general, closely anchyclosed together. The lower pieces have each a re-entering angle, in their upper edges, for the reception of the upper pieces which stand upon them. This structure was first pointed out by Mr. Lyon (Geol. Ky., vol. iii, p. 468), and is not generally admitted, although I believe it certainly does exist. It is said that the lower pieces consist of the upper joint of the column, divided into three by vertical sutures. To me they appear to be calycine plates. It is true that they do not form the bottom of the visceral cavity, but this may be due to the growth inward of the lower edges of those of the upper series. Something like this occurs in *Antedon*, where, at first, the bottom of the cup is formed by the basals, but afterwards principally by the first radials.

The forked plates are usually called "*Radials*," but they certainly do not belong to the radial system. If they did, they would represent the first radials of the Crinoidea, and therefore they should support the bases of the ambulacra. A little consideration will, however, enable any one to perceive that in *Pentremites* the bases of the ambulacra, are situated in the apex

of the fossil, and do not come in contact with the forked plates. The apex of *Pentremites* is identical with the actinal center of Sea-urchins and Star-fishes, in which the mouth is situated. It is here that the ambulacra originate and grow outward by the addition of new plates to their distal extremities. There can be little doubt that such was the mode of growth of the ambulacra of the *Pentremites*. The smaller extremity, therefore, of their ambulacra, which is received into the forked plate, is not the base, but corresponds with the apex of the ambulacrum of a Sea-urchin or of a Star-fish. It also represents the tip of the arm of a Crinoid. If the forked plate is radial, then the arrangement of the ambulacrum must be the same as that which would be exhibited in a Crinoid, with the upper end of the arm downward, and resting on the first radial, whilst the lower end would be upward, the tip being formed of the second radial. From this it follows that the forked plates do not belong to the radial, but to the perisomatic system.

The five deltoid plates alternate with the forked plates, and are also perisomatic.

It is not certain that the lancet plates represent any of those plates which in the Crinoidea are usually called "radials." They are so arranged that if they were loosened from the walls of the cup, and their smaller extremities turned upward, whilst their bases or larger ends retained their position, they would stand in a circle around the apex, as do the arms of an ordinary Crinoid. Their bases would alternate with the apices of the deltoid plates. They would form the outside of the arms, whilst the grooves and pinnulæ would be inside. Each would bear, on its outer or dorsal aspect, two elongated sacks, the two hydrospires that belong to the ambulacrum. I believe that the small groove in the ambulacrum of *Pentremites* was occupied by the ovarian tube only. If this be true, and if, also, the lancet plates represent the radial plates of the arms of the Crinoids, then the arm of *Pentremites* would have the respiratory portion of the ambulacral system on its dorsal, and the ovarian portion on its ventral aspect.

In the true Crinoids, both the respiratory and ovarian tubes are situated in the groove in the ventral side of the arm.* In

* Thomas Say, who was the first to recognize the Blastoidea as a group distinct from the Crinoidea, also supposed the function of the ambulacra to be respiratory. He says, "I think it highly probable that the branchial apparatus communicated with the surrounding fluid through the pores of the ambulacræ, by means of filamentous processes; these may also have performed the office of tentacula, in conveying food to the mouth, which was, perhaps, provided with an exsertile proboscis; or may we not rather suppose that the animal fed on the minute beings that abounded in the sea water, and that it obtained them in the manner of the Ascidia, by taking them in with the water. The residuum of digestion appears to have been rejected through the mouth." (Jour. Acad. N. S. Phil., vol. iv, p. 296, 1825).

the Crinoids the pinnulæ are attached to the radial joints of the arm. In *Pentremites* they are not connected with the lancet plate, but with the pore plates. In *P. pyriformis* they appear to me to stand in sockets excavated in the suture between the pore plates proper, and the supplementary pore plates. Miller compared them to the series of azygos plates, which underlie that portion of the ambulacrum of *Pentacrinus* that runs from the mouth to the base of the arm. These resemble the lancet plates, in their being azygos and not connected with pinnulæ; but then, on the other hand, they differ from them in having, a portion at least, of the respiratory tubes on their ventral aspect. Mr. Rofe says that, "in many species of Pentremite, if not in all, this lancet plate is in reality a compound plate, formed of two contiguous plates, extending from the bottom of the sinus to the top, and, then turning right and left round the summit-openings, they pass down the adjoining sinus, to form half its lancet-plate, leaving at the apex of the body a pentagonal aperture, supposed to be the mouth. In some weathered specimens, the two parts of the lancet plate are separate; and in many they appear to meet only at the top and bottom of the cross section, leaving a lozonge-shaped opening between them." (Geol. Mag., vol. ii, p. 249.) In a large specimen of *P. obesus* (Lyon and Cassiday) which was given to me by Mr. Lyon, a polished section shows that one of the lancet plates is thus divided, but in general no trace of a suture can be seen in these plates.

There are several points in the structure of the ambulacra of *Pentremites* that are well worthy of the study of those who have plenty of well preserved specimens. Among these, I would direct special attention to the markings in the ambulacrum of *P. pyriformis*. The median groove, which I suppose to have been exclusively occupied by the ovarian tubes, sends off branches, right and left alternately, toward the sides of the ambulacrum. These branches do not run directly to the ambulacral pores. Each of them terminates at a point between the inner extremities of two of the pores. There is at this point a small pit which appears to be the socket of an appendage quite distinct from the pinnule. The groove does not reach the socket of the pinnule, which is situated further out, between two of the pores. On the other hand a small groove runs from each pore, inward, and terminates at another socket, about half-way between the pore and the main median groove of the ambulacrum. It would thus appear that besides the ordinary pinnules, there were two other rows of appendages on each side of the median groove.

The general conclusions at which I have arrived from the above, are, that all the principal plates that compose the shell

of *Pentremites*, belong to the perisomatic system of Professor Wyville Thompson; that it is doubtful whether or not the lancet plates are homologous with the radial plates of the Crinoids; and that the ambulacra are more complicated in their structure than is generally supposed.

7. *On the Structure of the genus Nucleocrinus.*

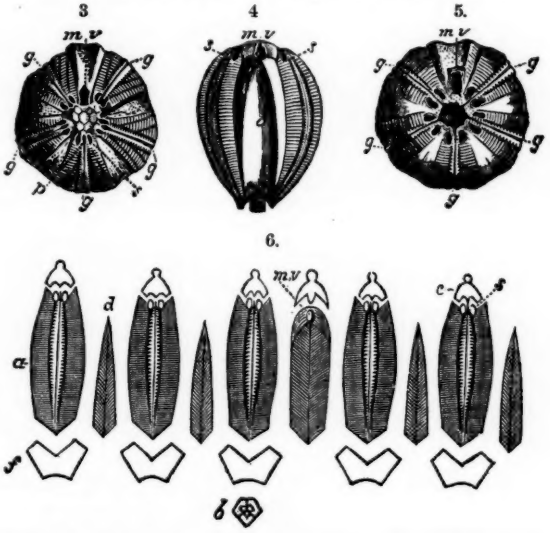


Fig. 3.—Apex of *Nucleocrinus Verneuilii* Troost. *g*, ambulacral groove; *p*, pore through which groove enters into the interior; *s*, one of the ten spiracles; *mv*, oro-anal aperture. 4. Anterior side of a specimen; *a*, the anterior interradial. 5. Apex of a specimen which has lost the integument that covered the center. 6. Diagram of the plates of the test; *a*, ambulacral plate; *b*, the basals; *c*, plates of the apex; *d*, one of the interradials; *f*, forked plate.

The body of this remakable genus is ovate, elliptical or oblong, and inclosed in a shell of strong perisomatic plates, which are, in general, so closely anchyclosed that the sutures between them cannot be distinguished. According to Mr. Lyon, who, through his long continued geological researches, has collected and studied a vast number of specimens, there are three minute lozenge-shaped, or quadrilateral basal plates, situated at the bottom of the columnal pit; always concealed when the column is present. These are surrounded by three other plates, the six altogether corresponding to the six pieces which constitute the compound basal plates of *Pentremites*. They are represented at fig. 6, *b*, as figured by Mr. Lyon (Geol. Ky., vol. iii, pl. v, fig 1, *b*.)

In the next series there are five plates which are undoubtedly the homologues of the five forked plates of *Pentremites*. They are very short and confined to the base of the body. They form a shallow basin with ten re-entering angles in its margin. Fig. 6, *f*.

Alternating above the forked plates, are five pieces corresponding to the deltoid or interradial plates of *Pentremites*. Some of these are lanceolate in form (fig. 6, *d*), their broader extremities fitting into the angles between the forked plates. They taper to a point upward, and their sides are bevelled so as to pass under the ambulacral plates, to which they are, in general, so closely united, that the line of junction is indicated only by the difference in the markings of the surface. Owing to this structure, these plates have not always been recognized by the authors who have described this genus. They were first pointed out by Mr. Lyon. The fifth deltoid or interradial plate is truncated at its apex for the reception of the ovo-anal orifice (*mv*, figs. 4, 6). The sutures on each side of this plate are generally distinctly visible, especially in the upper part of the body.

The ambulacra are narrow—one line wide in a specimen fifteen lines in length, with a fine median groove, about large enough to accommodate a tube of the size of a horse-hair. There are two rows of pores, those on one side of the groove alternating in position with those on the other side. These pores lead into the hydrospires. There appear to be only two rows of ambulacral ossicles. The pores are situated in the sutures between them. On each side of the ambulacrum there is a broad transversely grooved marginal plate. From each pore a small rounded ridge runs across this plate. The grooves between the ridges originate at the outer extremities of the ambulacral ossicles. In well-preserved specimens the surface of these marginal plates exhibits no other structure than the transverse grooves and ridges; but in one weathered specimen that I have examined, they seem to be composed of a number of narrow elongated pieces, arranged transversely, in such a manner that two of them abut against the outer extremity of each of the ambulacral ossicles, and extend outward toward the interradials. This seems to prove that the marginal plates belong to the ambulacra, as pointed out by Mr. Lyon, and not to the interradials, as represented by other authors. Although I have studied a large number of specimens, none of them were sufficiently perfect to enable me to make out the whole structure of this part of the test of *Nucleocrinus*. I have, however, seen enough to convince me that the ambulacra are much more complex than is usually supposed. The lancet plate, if it occur at all in this genus, must be very narrow. The ambulacral groove, as in *Pentremites*, sends off branches, right and left.

There is also evidence of the existence of minute marginal plates on each side of the groove.

7.

Fig. 7. Transverse section through a specimen which has all the hydrospires preserved. *h*, the two anterior hydrospires; *p*, pore leading into the hydrospire; *g*, one of the grooves.

The hydrospires are ten elongated sacks, each with two deep folds. They are perfectly homologous with those of *Pentremites*, only differing therefrom in not being united in pairs; consequently there are ten spiracles instead of five. The mouth, or oro-anal orifice, is larger in proportion to the size of the body than it is in *Pentremites*. Mr. Meek informs me that the mouth in some of the Blastoidea is protected by a single valve that covered it like the lid of a jug. From the structure of the orifice, I am inclined to think that in *Nucleocrinus* it possessed a similar protection.

In the apex, nearly all the space within the circle of apertures is covered by a thin integument of small plates, fig. 3. When this is not preserved, a large sub-pentagonal aperture is seen, as shown in fig. 5. This aperture occupies the position of the mouth in the existing echinoderms. The integument, as will be shown further on, represents that which covers the mouth of an embryonic Star-fish. Mr. Conrad described this genus in 1842, as having only one aperture in the summit. "This genus differs from PENTREMITES, Say, in having only one perforation at top, which is central." (Jour. Acad. Nat. Sci. Phil., vol. viii, p. 280, pl. xv, fig. 17). His figure represents the fossil with the apex downward. Dr. Ferd. Roemer, showed that, when perfect, there is no central opening, and he made this one of the grounds for separating the genus from *Pentremites*. He described the apex as being provided with six apertures, five of which were divided by a partition within each. These he considered to be the ovarian orifices. The sixth he supposes to be both mouth and vent, which accords with my view. (Mon. der Blastoideen, p. 378). In 1868 I discovered the five small pores at the apical extremities of the ambulacral grooves. (This Jour., II, xcvii, p. 353, and Annals Nat. Hist., IV, vol. 4, p. 76). In general it is difficult to see these pores, but if a silicified specimen, which has been fossilized in a calcareous matrix, be placed in an acid for two or three minutes, the acid cleans them out and they then become distinctly visible. I believe these to be the pores through which the ovarian tubes passed outward along the grooves to the pinnulæ. There are thus, sixteen apertures in the apex of *Nucleocrinus*,—ten spiracles, five ovarian orifices, and one oro-anal aperture. There are no true radial plates. The whole of the test with the exception, perhaps, of the ambulacra belongs to the perisomatic system.

8. *On the occurrence of Embryonic forms among the Paleozoic Echinoderms.*

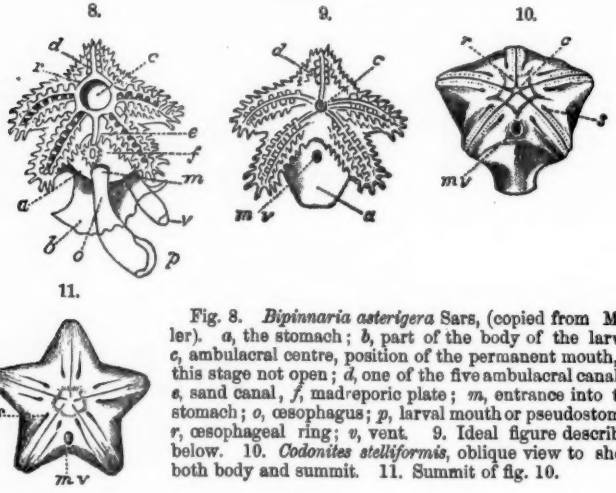


Fig. 8. *Bipinnaria asterigera* Sars, (copied from Müller). *a*, the stomach; *b*, part of the body of the larva; *c*, ambulacral centre, position of the permanent mouth, in this stage not open; *d*, one of the five ambulacral canals; *e*, sand canal, *f*, madreporic plate; *m*, entrance into the stomach; *o*, œsophagus; *p*, larval mouth or pseudostome; *r*, œsophageal ring; *v*, vent. 9. Ideal figure described below. 10. *Codonites stelliformis*, oblique view to show both body and summit. 11. Summit of fig. 10.

No proposition in Natural History has been more clearly demonstrated than this:—That, in general, the paleozoic animals resemble, both in external form and internal structure, the embryonic stages of those of the same class at present existing. Prof. Agassiz has long taught in his lectures and various publications, that this is especially observable in the Echinodermata. Judging from the figures and descriptions of Müller, Agassiz, Thomson, Carpenter and others, I should say, that in this class, the most striking resemblance is that which occurs between the adult stages of the Cystidea, Blastoidea, and Crinoidea, on the one hand, and the embryonic Star-fishes on the other. The structural character that has the most important bearing on the subjects discussed in these notes, is, that in all four of these groups, the mouth is situated in one of the interradial areas,—not in the ambulacral center, as it is in the adult forms of the existing Echinodermata.

In *Bipinaria asterigera* Sars, according to Müller, the digestive cavity is a sub-globular sack without any extensions into the rays, as there are in the adult Star-fishes. The œsophagus, fig. 8, *o*, is a fleshy, consistent tube, with a large mouth or pseudostome, *p*. It passes through the wall of the stomach by an opening somewhat smaller than the mouth, and situated in one of the interradial spaces at *m*. The madreporic plate, *f*, and sand canal, *e*, the latter holding the convoluted plate (when it

occurs), are situated above the orifice, *m*, and between it and the ambulacral center, *c*. The circular space at *c*, is undoubtedly the homologue of the central space in the apex of *Nucleocrinus*, figs. 3 and 5, and of *Codonites*, figs. 10 and 11. It is also the position of the mouth in the adult Star-fish; but in the larval stage it is completely closed by the soft external skin and sarcode of the body. In the fossils it is also closed, but by an integument of thin calcareous plates. The *Bipinnaria* is nourished by minute particles of matter diffused through the water, and drawn into the digestive sack through the mouth and œsophagus by the action of interradial cilia. I believe that all the fossil Crinoidea, Blastoidea and Cystidea, ingested their food in this way, and without any aid whatever from the arms or pinnulæ.

Perhaps there is no embryologist who will not admit, that it is possible for an animal like *Bipinnaria* to develope organs of reproduction and propagate its species, none of its other parts making any farther advance. Such an animal, with some slight modifications, would not be very widely different from a paleozoic Crinoid. If the sarcodic body wall were to be consolidated into a thin calcareous integument, with the mouth even with the surface, the swimming appendages aborted, and the vent closed up, it would resemble the cup of an *Actinocrinus*, fig. 9, *a*. The lateral orifice would then be both mouth and vent, as it is, at first (according to Prof. A. Agassiz, Seaside Studies, p. 125), in the embryo of *Asteracanthion Berylinus*. The ambulacral canals of *Bipinnaria* are the homologues, in a general way, of those which are found beneath the vault of *Actinocrinus*, and extend out into the grooves of the arms. If the ventral perisome of the Crinoid were to be removed (the internal organs remaining undisturbed) the arrangement disclosed would be that represented in fig. 9,—a convoluted plate in the center with the canals radiating from it. The most striking difference is the absence of the œsophageal ring. According to the organization of *Actinocrinus* there could be no œsphagus at that point, and consequently there is no ring. The convoluted plate represents the madreporic apparatus. The sucking feet of the Star-fish, most probably, represent the respiratory tentacles that border the grooves of the Crinoids, but modified into prehensile and locomotive organs. *Bipinnaria* and *Actinocrinus* agree in having the mouth in one of the interradial areas, and in the absence of an orifice through the perisome at the ambulacral center. These two characters are embryonic and transitory in the Star-fish, but they were permanent in most paleozoic Crinoids.

In *Codonites stelliformis* (*Pentremites stelliformis* Owen and Shumard), figs. 10, 11, the ambulacral center, *c*, is completely

closed. Five minute grooves radiate out to the extremities of the five angles of the disc. These grooves are identical with those of *Pentremites* and *Nucleocrinus* and were occupied by the ovarian tubes. The ambulacral canals of the true Crinoids and of the Star-fishes are represented in a rudimentary condition, in this species, by the hydrospires which open out to the surface through the ten fissure-like spiracles, *s*. The oro-anal orifice is interradial. *C. stelliformis* in external form, the interradial position of the mouth, and the closed ambulacral center, resembles *Bipinnaria* and *Actinocrinus*, but differs importantly in having its respiratory organs arranged in ten separate tracts, all totally disconnected from each other. It is a lower form than *Actinocrinus*, which in its turn is lower than *Bipinnaria*, and yet all three are constructed on the same general plan.

C. stelliformis, although much resembling a *Pentremite*, is a true Cystidean. Its affinity to *Codaster* was first pointed out by Dr. C. A. White, who also suggested that it should be assigned to a distinct group. (Bost. Jour. N. H., vol. vii, pp. 486, 487). The main difference between the Cystidea and the Blastoidea is, that in the former the hydrospires do not communicate with the pinnulæ, whilst in the latter the cavities of the pinnulæ and hydrospires are directly connected by the ambulacral pores.

The developement of the recent Crinoid *Antedon rosaceus*, as described by Prof. Wyville Thomson (Phil. Trans., 1866), pursues a course that could not possibly result in the production of such an animal as *Actinocrinus*. The pseudembryo, as it is called by Prof. Thomson, is a small ovate organism, with four transverse ciliated bands, a large key-hole-shaped mouth (pseudostome), and a small circular vent (pseudoproct). These orifices are connected by a rudimentary intestine (pseudocele). In this stage there is no trace of radiation, and the mouth, therefore, cannot be said to be interradial in its position.

The nascent Crinoid originates within the pseudembryo, but developes a mouth, vent and stomach, of its own, all quite distinct from those of its nurse. This new, or permanent mouth, is for a short time both oral and anal in its function, but although in this respect it resembles that of *Actinocrinus*, its position in the center of the ambulacral system, shows it to represent the mouth of the adult Star-fish, while that of *Actinocrinus* rather homologates with the oral orifice of the *Bipinnaria*. At no time during its development does the ventral perisome exhibit the structure of that of the paleocrinoids, i. e., no orifice in the ambulacral center, and at the same time one in an interradial space. In the central position of its mouth, and in the possession of an œsophageal ring, *Antedon* stands

above *Actinocrinus* in rank, and between it and the adult Starfish. In none of its stages does it resemble a *Bipinnaria* either in form or in structure.

9. *On some of the objections that have been advanced against the views advocated in the preceding notes.*

In all the known species of the existing Echinodermata, the mouth is situated in the center of the ambulacral system, and it is contended that this fact proves that such must have been its position also in the paleozoic forms.

This reasoning is not strictly logical. It is true that in the known existing species, the mouth is in the center, but it does not certainly follow that it is so in all the Echinodermata, living and extinct. Whether it be so or not in any particular fossil species whose structure may be under investigation, *is a question of fact which can only be positively determined by direct observation of specimens.* On appealing to these we find that, in a large proportion of the fossil forms, there is no aperture in the perisome at the ambulacral center. It also becomes evident by the comparison that, in general, the paleozoic species resemble the embryonic stages of some of the recent Echinoderms, and that in these, (*Bipinnaria* for instance), the mouth is interradial. Rules such as is relied on in this case, afford a certain amount of presumptive evidence, which, however, cannot prevail against material and visible facts. When we can see clearly that there is no aperture in that point, in the vault of a Crinoid, beneath which we know the ambulacral center is situated, it is perfectly useless to supply one by deduction.*

The second objection is, that many of the fossils have a *Platyceras* attached to them, in such a position as to cover the aperture which I call the mouth, and under such circumstances as to induce the belief that it lived parasitically on the Crinoid. The only answer I can make to this is that, admitting the facts, we must suppose that space was left for a stream of water to pass under the edge of the shell, into the mouth of the Crinoid. In general, where one animal lives parasitically upon another, it does not destroy its host. Some of the gasteropods of the Devonian and Carboniferous ages, were carnivorous, as is proved by the bored shells and Crinoids that are occasionally found. I have seen a number of such specimens, and several

* The position of the ambulacral center may thus be found. When the mouth is eccentric, the ambulacral tubes usually converge to the center of the vault. But when the mouth is central, we first find the azygos interradius, in general easily recognized by its possessing a greater number of plates than do any one of the other four interradii. On the opposite side of the fossil is the azygos arm. The ambulacral center is always situated between this arm and the mouth, never on the side of the mouth toward the azygos interradius.

years ago I read a paper on the subject (which was never published) before the Natural History Society of Montreal. There were several good Conchologists present, and the specimens exhibited were compared with bored shells of existing species. All pronounced the style of workmanship to be precisely the same. I have the proboscis of an *Actinocrinus* that is bored near the base, and among the fossils lent me by Mr. Wachsmuth, is a *Codonites stelliformis*, that is bored through one of the ambulacra. The view I took of the subject in my paper, was that the gasteropod ascended the stalk of the Crinoid, and thrust its proboscis into the mouth of the latter. The Crinoid then slowly drew its arm together, and held the shell fast until both died.

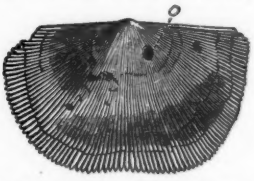


Fig. 12. *Streptorhynchus Pandora.* A specimen bored at *o* by a carnivorous gasteropod. From the Corniferous Limestone, Devonian, Canada.

A third objection is the small size of the aperture in some of the species. In general, where there is no proboscis, the orifice is from one-twentieth to one-tenth of an inch in diameter, quite sufficient for an animal that subsists on microscopic organisms. It is stated by Meek and Worthen that where there is a proboscis, the aperture is sometimes scarcely "more than *one-hundredth* of an inch in diameter." I believe that in many such instances the tube filled up by calcareous deposits on its inside, and that when entirely obstructed, either a new aperture opened out in the side of the proboscis, or that the animal died. In Mr. Wachsmuth's collection, I saw a specimen with a second aperture in process of formation. A ticket was attached to it by him, giving this explanation. I am also informed that in some of the existing species of *Antedon* "the mouth is an exceedingly minute aperture."

A fourth objection is that the aperture is so situated that the arms could not have conveyed food to it. It is, however, proved by Dr. W. B. Carpenter, that in the recent Crinoids the arms are not prehensile organs. The animal while feeding remains motionless, attached by its dorsal cirrhi to a stone, shell, or other object on the bottom. Its arms are either stretched out to their full length, or more or less coiled up, but quite immovable. As Dr. Carpenter's remarks have a very important bearing upon the subject, I shall take the liberty of quoting the following:—

"Whatever may be the purpose of the habitual expansion of the arms, I feel quite justified that it is *not* (as stated by several authors whom I have cited in my historical summary) the prehension of food. I have continually watched the results of

the contact of small animals (as Annelids, or Entomostracans and other small Crustaceans) with the arms, and have never yet seen the smallest attempt on the part of the animal to seize them as prey. Moreover, the tubular tentacula with which the arms are so abundantly furnished, have not in the slightest degree that adhesive power which is possessed by the "feet" of the ECHINIDEA and ASTERIADA; so that they are quite incapable of assisting in the act of prehension, which must be accomplished, if at all, either by the coiling-up of a single arm, or by the folding-together of all the arms. Now I have never seen such coiling up of an arm as could bring an object that might be included in it into the near neighborhood of the mouth; nor have I seen the contact of small animals with a single arm produce any movement of other arms towards the spot, such as takes place in the prehensile apparatus of other animals. Moreover, any object that could be grasped either by the coiling of one arm, or by the consentaneous closure of all the arms together upon it, must be far too large to be received into the mouth, which is of small size and not distensible like that of the ASTEROIDA."*

Farther on Dr. Carpenter says:

"It was affirmed by M. Dujardin (l'Institut, No. 119, p. 268) that the arms are used for the acquisition of food in a manner altogether dissimilar to ordinary prehension; for recognizing the fact that the alimentary particles must be of small size, he supposed that any such, falling on the ambulacral (?) furrows of the arms or pinnæ, are transmitted downwards along those furrows to the mouth wherein they all terminate, by the mechanical action of the digitate papillæ which fringe their borders. This doctrine he appears to have abandoned; since in his last account of this type (Hist. Nat. des Echinoderms, p. 194) he affirms that the transmission of alimentary particles along the ambulacral (?) furrows is the result of the action of cilia with which their surface is clothed. Although I have not myself succeeded in distinguishing cilia on the surface which forms the floor of these furrows, yet I have distinctly seen such a rapid passage of minute particles along their groove as I could not account for in any other mode, and am therefore disposed to believe in their existence. *Such a powerful indraught, moreover, must be produced about the region of the mouth, by the action of the large cilia which (as I shall hereafter describe) fringe various parts of the internal wall of the alimentary canal, as would materially aid in the transmission of minute particles along those portions of the ambulacral (?) furrows which immediately lead toward it;* and it is, I feel satisfied, by the conjoint agency of these two moving powers that the alimentation of Antedon is ordinarily affected. In the very numerous specimens from Arran the contents of whose digestive cavity I have examined, I have never found any other than microscopic organisms;

* Researches on the Structure, Physiology, and Development of *Antedon* (*Comatula*, Lamk.) *rosaceus*.—Part I. By W. B. Carpenter, M.D., F.R.S. Philosophical Transactions of the Royal Society, vol. clvi, Part II. 1866

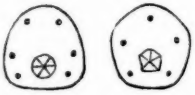
and the abundance of the horny rays *Peridinium tripos* (Ehr.) has made it evident that in this locality that Infusorium was one of the principal articles of its food. But in Antedons from other localities, I have found a more miscellaneous assemblage of alimentary particles; the most common recognizable forms being the horny casings of ENTOMOSTRACA or of the larvæ of higher CRUSTACEA." (Op. cit., p. 700).

The existence of large cilia within the intestinal canal, capable of producing a powerful indraught of water, renders any movement or concurrent action of the arms quite unnecessary in the ingestion of food. It does not matter, therefore, in what part of the body the mouth of a Crinoid may be situated, or how remote from the reach of the arms. Attached permanently to the bottom of the sea by their columns, the paleozoic Crinoidea, Cystidea and Blastoidea remained, while feeding, most probably motionless, drawing in streams of water through their mouths by the action of their intestinal cilia. The long tubular proboscis, with which many of the species are provided, would be, thus, analogous in function to the siphon of the acephalous mollusca. The indigestible particles would be, from time to time, thrown out through the mouth, just as a Star-fish or a Zoophyte frees itself of the refuse portions of its food, by casting it out of the same aperture through which it entered.

10. *On the Theory that the ambulacral and ovarian orifices are the oral apertures.*

Assuming that the four objections above noticed are sufficient to prove that the aperture which I call the mouth is not that organ, it is contended that the Cystidea, Blastoidea and Palæocrinidea ingested their food through their ambulacral and ovarian orifices. This appears to me in the highest degree improbable. In the recent Crinoids the grooves of the arms are occupied by four sets of tubes, which Dr. Carpenter calls the cœliac, the sub-tentacular, the ovarian and the tentacular canals. None of them communicate with the stomach. It is impossible that the most minute particle of food could gain access into the interior of the animal through any of them. The structure of the arms of the paleozoic Crinoids is such, that we must presume that their grooves were occupied by similar tubes, which passed through the ambulacral orifices into the perivisceral space. In the Cystidea and Blastoidea the respiratory organs were not situated in the grooves of the arms, and the ambulacral orifices were therefore only ovarian in their function. The improbability of their being also oral apertures is best shown by an illustration.

13. 14.



In fig. 13, is represented (natural size) the apertures of the smallest specimen of *Caryocrinus ornatus*, in our collection, selected for the present purpose because in the young of this species, the valvular orifice is larger in proportion to the size of the disc, than it is in the adult. It is in this specimen, about one-third of the whole width of the apical disc, while in a full grown *Caryocrinus* it is only one-ninth of the width. The same proportional size of the mouth according to age, occurs in *Antedon rosaceus*. The valvular mouth at first is as wide as the disc. But as the age of the animal increases the disc grows wider but the mouth does not. The ovarian pores in *Caryocrinus* are, however, as large in the small ones (once they make their appearance) as they are in those full grown. For recognizing these as ovarian pores we have the following reasons:— 1. They are situated at the bases of the arms where the ovarian tubes must pass from the grooves into the perivisceral cavity. 2. When compared with the ovarian pores of a Sea-urchin they have the same size, form and aspect. Fig. 14, represents the ovarian pores of the Sea-urchin *Toxopneustes Drobachiensis* Ag. natural size and arrangement. It may not appear at first view that this latter comparison has any probative effect. But it has, in this way. If these apertures in *Caryocrinus* were large openings a line wide, as are some of the ambulacral orifices of the Crinoids, I would say that they were unlike true ovarian apertures.

According to the new theory, this Echinoderm *Caryocrinus ornatus* was a polystome animal, and drew in its food through its six ovarian apertures, the large valvular orifice being the anus. To me this appears to be utterly incredible.

In fig. 14 I have represented the mouth of *Leskia mirabilis* Gray. Both Dr. I. E. Gray and Prof. Lovén have pronounced this aperture to have the structure of the valvular orifice of the Cystidea. I have not the slightest doubt whatever but that the mouth of the Cystideans foreshadows that of the Sea-urchins. There is nothing whatever in its structure to show that it is not the mouth but on the contrary.

The new theory is not founded upon any peculiarities in the structure of the ambulacral orifices, which would show that they are oral apertures, but only upon the four objections above noticed. The first of these is not logical, while at the same time it is purely theoretical, and avails nothing against material and visible facts. The fourth is completely disposed of by Dr. Carpenter's observations, which prove that in the Crinoidea the arms have no share whatever in the ingestion of food. The second and third objections are the same in substance, i. e., according to the second the supply of water to the

mouth, is diminished by the occurence of a *Platycerus* over it, while, according to the third, the same effect is produced by the small size of the aperture itself in some instances. It does not require much consideration to convince one, that if these two objections are fatal to my views, they are equally so to the opposite theory. In *C. stelliformis*, for instance, the pores through which we must suppose the ovarian tubes issued from the interior are only large enough to admit of the passage of a fine hair. They are scarcely visible to the naked eye. The tube, under any circumstances, must have filled them almost entirely. If any space at all were left for the passage of a stream of water through the pore by the side of the tube it must have been exceedingly minute.

When weighed as above, therefore, the evidence gives the following results:—The first and fourth objections avail nothing. The second and third militate against both theories. But when we take into account that in no instance, in the existing Echinodermata, where ovarian pores occur, are they at the same time oral orifices, the balance seems to be in favor of my view. This is all I desire to say upon the subject at present. Although I now firmly believe that the valvular orifice in the Cystidea, the larger lateral aperture of the Blastoidea, and the so-called proboscis of the paleozoic Crinoids are all oro-anal in function, yet I shall not maintain that view obstinately against good reason shown to the contrary.

ART. XXIV.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School.* No. 12.

§ 1.

On the precipitation and determination of the metals of the magnesium group in the form of oxalates; by W. GOULD LEISON.

PROF. GIBBS has recently* called attention to the fact that a number of metallic oxyds may be completely precipitated from their neutral solutions by means of oxalic acid, provided that a large excess of alcohol be also added. As it is not easy to obtain precise quantitative results by igniting the oxalates so precipitated, in consequence of the extreme subdivision of the resulting oxyds, Prof. Gibbs suggested the employment of potassic hypermanganate for the combustion of the oxalic acid, a method which—as is well known—gives excellent results in the case of calcic oxalate precipitated in the ordinary manner. The following investigation was undertaken for the purpose of testing this method of analysis:—

* This Journal, xliv, p. 213.

Cadmium.—Cadmic sulphate was dissolved in the least possible quantity of water, oxalic acid added in excess, and then a large quantity of strong alcohol. The resulting oxalate was beautifully crystalline, and the precipitation was so complete that SH_2 gave in the filtrate a scarcely perceptible yellowish tinge. The oxalate was washed with alcohol by Bunsen's method and dried at 110° C., until every trace of alcohol was expelled. The filter was then pierced with a glass rod, and the cadmic oxalate washed into a flask with hot diluted sulphuric acid. A few cubic centimeters of strong sulphuric acid were then added, and the hot solution titrated with potassic hypermanganate. In this manner four experiments gave 44·19 pr. ct., 44·65 pr. ct., 44·88 pr. ct., and 44·27 pr. ct. of cadmium, as computed from the oxalic acid. These results are all much too high, and show that the acid had acted sensibly upon the filter. Two other experiments were then made. In the first a hot solution of ammonic sulphate was used as a solvent for the oxalate; in the second hot dilute chlorhydric acid was employed. Of the hypermanganic solution employed 100 c. c. contained 0·1103 gr. of available oxygen.

I. 0·4330 gr. cadmic sulphate required 24·5 c.c. hypermanganate = 43·68 pr. ct. Cd.
II. 0·3724 gr. cadmic sulphate required 21·1 c.c. hypermanganate = 43·74 pr. ct. Cd.

The received formula $3CdSO_4 + 8H_2O$ requires 43·75 pr. ct. In these two analyses the filters were not broken.

Barium.—Baric chlorid gave extremely variable results in my first experiment, notwithstanding the fact that the barium is completely precipitated by oxalic acid and alcohol. The resulting oxalate, after washing and drying, was not completely decomposed by sulphuric acid, which appeared to form a crust of baric sulphate upon the crystals of the oxalate. This difficulty was finally overcome by dissolving the baric oxalate in chlorhydric acid and diluting the solution largely. In this manner:

0·6505 gr. baric chlorid required 80 c.c. hypermanganate = 56·21 pr. ct. Ba (100 c.c. hypermanganate solution contained 0·053 gr. available oxygen). The formula $BaCl_2 + 2H_2O$, requires 56·15 pr. ct. Ba.

Strontium.—To avoid the use of paper filters so as to be able to employ sulphuric acid as a solvent, I resorted to sand filters. A light funnel was ground truly conical near the throat. A little pear of glass with a long stem was then dropped into the funnel, stem upward. In this manner a valve was formed impassible to the sand laid upon the ball of the glass, but allowing liquids to pass freely. By means of the stem the valve

could be lifted from its seat, and the sand and precipitate washed together into a flask, after careful drying. With this arrangement:

0·4292 gr. strontic nitrate required 47·8 c.c. hypermanganate = 48·90 pr. ct. SrO.

0·3657 gr. strontic nitrate required 40·8 c.c. hypermanganate = 48·90 pr. ct. SrO. (100 c.c. hypermanganate solution contained 0·1099 gr. available oxygen.)

The formula $Sr(NO_3)_2$ requires 48·93 pr. ct. SrO. Sulphuric acid only was used to decompose the oxalate.

Calcium.—Iceland spar was dissolved in chlorhydric acid, and the solution treated with oxalic acid and alcohol. The filtrate contained calcium. When, however, the solution was evaporated to dryness before adding alcohol, and the oxalate was washed on a sand filter, no traces of calcium could be detected in the filtrate. In this manner:

0·5090 gr. $CaCO_3$ required 70·6 c.c. hypermanganate = 56·10 pr. ct. CaO. (100 c.c. hypermanganate corresponded to 0·11559 gr. oxygen.)

0·5590 gr. $CaCO_3$ required 77·5 c.c. hypermanganate = 56·08 pr. ct. CaO. (100 c.c. hypermanganate corresponded to 0·11495 gr. oxygen.)

The formula requires 56·00 pr. ct. CaO. Sulphuric acid only was employed.

Magnesium.—When magnesic sulphate is treated with oxalic acid, the mixture evaporated, but not to dryness, and alcohol added, the filtrate is perfectly free from magnesium. In this manner:

0·3243 gr. $MgSO_4 + 7H_2O$ required 39·6 c.c. hypermanganate = 16·18 pr. ct. MgO.

0·3949 gr. $MgSO_4 + 7H_2O$ required 48·4 c.c. hypermanganate = 16·25 pr. ct. MgO.

In these analyses the oxalate was collected on a paper filter and washed into the flask with water after piercing the filter, which was washed with cold dilute sulphuric acid. The formula requires 16·26 pr. ct.

Zinc.—Zinc is completely thrown down from its sulphate by the unmodified process. The oxalate forms an extremely fine powder. A sand filter and warm dilute sulphuric acid were employed.

0·9301 gr. sulphate required 47·1 c.c. hypermanganate = 28·14 pr. ct. ZnO.

1·0788 gr. sulphate required 54·6 c.c. hypermanganate = 28·15 pr. ct. ZnO.

The formula requires 28·22 pr. ct. ZnO.

Cobalt.—Perfectly pure anhydrous cobaltous chlorid was prepared by igniting chlorid of purpureo-cobalt. The chlorid was then precipitated by oxalic acid and alcohol, collected on a sand filter and digested with dilute sulphuric acid. The solution was intensely red. A solution of nickelous sulphate was then added, until the red color disappeared and a faint smoky hue took its place.* In this manner:

0·4292 gr. $CoCl_2$ required 47·8 c.c. hypermanganate = 45·30 pr. ct. Co.

0·3657 gr. $CoCl_2$ required 40·8 c.c. hypermanganate = 45·37 pr. ct. Co.

The formula requires 45·38 pr. ct. (Co = 59).

Nickel.—In the case of nickelous sulphate it was found necessary, after adding the oxalic acid, to concentrate the mixture on a water bath before adding alcohol, and then further digest for about half an hour, replacing the alcohol as fast as it evaporated. The oxalate was collected on a paper filter, and, after washing, dissolved in ammonia on the filter. The filtrate was then acidified with sulphuric acid, and the color finally discharged by a solution of cobaltous sulphate. In this manner:

0·9585 gr. nickelous sulphate required 42·2 c.c. hypermanganate = 28·57 pr. ct. NiO.

1·0287 gr. nickelous sulphate required 45·3 c.c. hypermanganate = 28·58 pr. ct. NiO.

The formula $NiSO_4 + 6H_2O$ requires 28·24 pr. ct. NiO (Ni = 58), but it is very difficult to obtain the sulphate in a perfectly definite state of hydration.

Manganese.—Although manganese is completely precipitated from its soluble salts by oxalic acid in the presence of alcohol, my results with the method have not been satisfactory, owing as I suppose to my not having a definite salt for analysis. The following analyses show at any rate that closely corresponding results can be obtained when the same substance is taken:

0·3760 gr. manganous oxalate required 30·50 c.c. hypermanganate = 38·38 pr. ct. MnO.

0·4013 gr. manganous oxalate required 32·55 c.c. hypermanganate = 38·38 pr. ct. MnO.

The salt $2C_2MnO_4 + 5H_2O$ when dried in air requires 37·77 pr. ct. MnO, while the salt analyzed was dried at 100° C. In like manner two analyses of a sulphate, which had probably absorbed a little water, gave 45·28 pr. ct. and 45·29 pr. ct. of MnO. The crystallized sulphate $MnSO_4 + 7H_2O$ requires 46·67 pr. ct.

* Compare, as regards this method, W. Gibbs, in this Journal, vol. xiv, p. 204.

Iron.—Good results could not be obtained by the application of this method to the determination of iron, but I am not at present able to assign the reason of the failure in this case.

To complete my work it remains for me to point out the applicability of the process to the determination of the whole quantity of oxygen contained in a number of bases present together in solution—a problem which is sometimes of interest.

Sulphates of Manganese, Magnesium, Nickel, Cobalt, Cadmium and Zinc in undetermined quantities were dissolved together in water, and the solution well shaken. Four portions, of 100 c.c. each, were then taken, and the acid determined in each by baric chlorid. In two other equal portions the bases were precipitated as oxalates and titrated as above. The oxygen in the acid was then calculated from the amonnt of baric sulphate. In this manner it was found that the oxygen in the acid was to that in the mixed bases as 3 to 1 very nearly, the precise ratio being in the one case as 0·054 is to 0·018 and in the other as 0·055 is to 0·018. Another experiment was made with a crystallized dolomite containing 0·45 pr. ct. of insoluble residue. The lime, magnesia and iron were precipitated together, and titrated as oxalates; the carbonic acid was determined by ignition. In this manner the oxygen of the acid was found to be to the oxygen in the bases as 34·48 is to 17·28. The bases after ignition amounted to 52·41 pr. ct.

In another experiment with a dolomite from a different locality containing 0·13 pr. ct. of insoluble residue, the oxygen ratio was found to be as 34·56 is to 17·28, the bases amounting to 52·33 pr. ct. If we calculate the relative quantities of calcic and magnesic carbonates from the sum of the two oxyds in the last analysis and the oxygen found by titrating the oxalate, we find:

$MgCO_3$ - - - - -	42·77
$CaCO_3$ - - - - -	57·07
Insol. residue - - -	0·13
	99·97

the small quantity of ferrous oxyd present being neglected. This analysis will serve to show the applicability of the method in indirect analyses. My results appear to me to furnish a generalization of the use of potassic hypermanganate, which will be received with favor by those who employ this reagent frequently in volumetric analyses.

§ 2.

On new analytical processes; by J. H. Talbott.

1. *On the precipitation of zinc and manganese as sulphids.*—Zinc is thrown down from cold solution by an alkaline sulphid in the form of a slimy mass which settles slowly and is ex-

tremely difficult to wash. The precipitation is, however, more complete than when sodic carbonate is used, and may be rendered very easy and rapid by the following process: The solution of zinc, if acid, is to be neutralized as nearly as possible by sodic or ammonic carbonate. To the boiling solution sodic or ammonic sulphid is to be added, a large excess being very carefully avoided. The white precipitate, on continued boiling, soon becomes granular, and settles readily. The supernatant, clear liquid is then to be tested with a drop of the alkaline sulphid, to be sure of complete precipitation, and the sulphid then washed with hot water by Bunsen's method. The filtrate is perfectly clear, and absolutely free from zinc; the washing is easy and rapid. The sulphid of zinc is then to be partially dried with the filter, in the manner recommended by Bunsen, brought into a porcelain crucible and ignited, at first gently, and afterward strongly with free access of air. The expulsion of the last traces of sulphuric acid is much facilitated by occasionally dropping fragments of ammonic carbonate into the crucible. Pure ZnO finally remains, the ignition being continued until a constant weight is obtained. In this manner the following results were obtained:

0·3216 gr.	pure	ZnO	gave	0·3216 gr.	= 100·00 pr. ct.
0·3208 gr.	"	"	"	0·3209 gr.	= 100·03 pr. ct.
0·2412 gr.	"	"	"	0·2410 gr.	= 99·91 pr. ct.
0·1785 gr.	"	"	"	0·1784 gr.	= 99·94 pr. ct.

In zincic sulphate, which had probably lost a little water:

0·6485 gr.	gave	0·1851 gr.	ZnO	= 28·54 pr. ct.
0·6510 gr.	"	0·1858 gr.	"	= 28·54 pr. ct.
0·8198 gr.	"	0·2338 gr.	"	= 28·52 pr. ct.

The formula requires 28·29 pr. ct. ZnO. The advantages of this process over the older methods of precipitating in the cold are, I think, very evident, even if only the saving of time be taken into consideration.

Manganese may be precipitated completely from its boiling solutions by precisely the same process. The flesh-colored sulphid is granular and sometimes even sandy, though not distinctly crystalline, and may be washed with the utmost facility. The precipitated sulphid, after washing upon a filter, is to be redissolved in chlorhydric acid, and precipitated as ammonio-phosphate in the manner proposed by Prof. Gibbs. To test the method with a perfectly definite salt of manganese, manganous pyrophosphate was selected, dissolved in dilute chlorhydric acid, and the solution nearly neutralized by sodic carbonate. To the boiling solution sodic sulphid was then added, and the manganese finally weighed—in one analysis as

pyrophosphate, in another as anhydrous sulphid by ignition in a currrent of SH_2. In this manner:

0·3132 gr. $Mn_2P_2O_7$ gave 0·3126 gr. $Mn_2P_2O_7$ = 49·56 pr. ct. MnO.

0·3786 gr. $Mn_2P_2O_7$ gave 0·2310 gr. MnS = 49·65 pr. ct. MnO.

The formula requires 49·64 pr. ct. MnO. It is perhaps worthy of notice that ammonic sulphid does not completely decompose manganous pyrophosphate under the circumstances above described. The greater portion of the salt is precipitated at once as crystalline ammonio-phosphate of manganese.

2. *On the quantitative separation of tin and tungsten.*

The quantitative separation of tin from tungsten has always been regarded as a difficult problem not hitherto solved in a satisfactory manner. The following method will, I think, be found to leave nothing to be desired as respects both ease and accuracy. It depends upon the fact that stannic oxyd, SnO_2, is reduced by potassic cyanid with great facility, while tungstic acid, WO_3, undergoes no reduction, even when heated with the cyanid to a high temperature. The oxyds of tin and tungsten are to be heated in a porcelain crucible with 3 or 4 times their weight of commercial potassic cyanid, previously fused, pulverized, and thoroughly mixed with the two oxyds. The mass is kept fused for a short time, when the tin separates in the form of metallic globules, while the tungstic acid unites with the alkali of the potassic cyanate and carbonate present. After cooling, the mass is to be treated with hot water, which dissolves the alkaline tungstate and other salts, and leaves the tin as metal. This is to be filtered off, washed, dried and weighed as stannic oxyd after oxydation in the crucible with nitric acid. The tungstic acid is most conveniently estimated by the difference, but may of course be precipitated by mercurous nitrate, after boiling the solution with nitric acid to decompose the excess of potassic cyanid present, and then redissolving the precipitated tungstic acid by means of an alkali. To test the method, weighed portions of pure stannic and tungstic oxyds were mixed and treated as above:

0·6662 gr. SnO_2 and 0·5880 gr. WO_3 gave 0·6679 gr. SnO_2 = 53·24 pr. ct. The calculated percentage of stannic oxyd is here 53·11.

0·7098 gr. SnO_2 and 0·5460 gr. WO_3 gave 0·7096 gr. SnO_2 = 56·51 pr. ct., the calculated percentage being 56·52.

0·5378 gr. SnO_2 and 0·4373 gr. WO_3 gave 0·5405 gr. SnO_2 = 55·43 pr. ct., the calculated percentage being 55·15.

0·5073 gr. SnO_2 and 0·4334 gr. WO_3 gave 0·5081 gr. SnO_2 and 0·4349 gr. WO_3. This corresponds to

	Found.	Calculated.
Stannic oxyd,	54·01	53·92
Tungstic oxyd,	46·23	46·08
	100·24	100·00

As it might perhaps be objected to the examples given above that I employed only purely mechanical mixtures of stannic and tungstic acids, and that this is not the case which occurs in practice, I made the following additional analyses: Portions of the two metallic oxyds were fused in a silver crucible with pure sodic hydrate; the fused mass was then dissolved in water and the two oxyds precipitated together from the solution by nitric acid with the usual precautions. The ignited mixed oxyds were then fused with potassic cyanid, as above. In this manner:

0·7292 gr. of a mixture of SnO_2 and WO_3 gave 0·4211 gr. SnO_2 = 57·74 pr. ct.
0·9826 gr. of the same gave 0·5661 gr. SnO_2 = 57·61 pr. ct.

Tin cannot be separated from molybdenum by fusing the mixed oxyds with potassic cyanid, as the molybdic acid is always more or less completely reduced to a lower oxyd.

§ 3.

On the treatment of gelatinous precipitates; by THOMAS M. CHATARD.

The inconveniences and loss of time which attend the washing of gelatinous precipitates are familiar to all chemists. Even the methods of washing recently introduced by Bunsen are not always perfectly satisfactory in their operation when applied to this class of substances. The following method will be found, I think, to give results which leave nothing to be desired: The solution containing the substance to be determined is to be simply evaporated to perfect dryness with a small excess of the precipitant, and the gelatinous mass stirred with a rod until it becomes a perfectly dry powder. In this manner the precipitate diminishes extremely in bulk, and may then be washed with the greatest ease upon the filter. The evaporation is usually effected with sufficient rapidity on a water bath. The following analyses will be sufficient to show the degree of precision attainable by this process in the cases of some of the more familiar precipitates: Weighed portions of potassic dichromate were dissolved in very small portions of water, reduced with chlorhydric acid and alcohol, the excess of alcohol expelled, and ammonia added in excess. After evaporation, in the manner above described, the chromic sesquioxyd presented a greenish blue granular powder very easily washed.

0·7782 gr. $K_2Cr_2O_7$ gave 0·4023 gr. Cr_2O_3 = 68·02 pr. ct. chromic acid.
1·5646 gr. $K_2Cr_2O_7$ gave 0·8102 gr. Cr_2O_3 = 68·13 pr. ct. chromic acid.

The formula requires 68·04 pr. ct.

Alumina treated in the same manner is also very easy to wash.

2·4097 gr. potassic alum gave 0·2626 gr. Al_2O_3 = 10·89 pr. ct.
1·9571 gr. " " " 0·2130 gr. " = 10·88 pr. ct.

The formula requires 10·86 pr. ct.

The process applies with almost equal advantage to iron. Weighed portions of ammonio-ferrous sulphate were dissolved in water, and sodic chlorid added in large excess to furnish solid matter to be washed out. The iron was then oxydized with nitric acid, precipitated by ammonia, and evaporated as above.

1·5824 gr. gave 0·3229 gr. Fe_2O_3 = 14·28 pr. ct. Iron.
1·4840 gr. gave 0·3019 gr. " = 14·24 pr. ct. Iron.

The formula requires 14·29 pr. ct.

Nickelous carbonate also loses its gelatinous character when treated as above. 0·2201 gr. metallic nickel gave, after solution, precipitation as carbonate, and reduction by hydrogen, 0·2199 gr. metallic nickel = 99·91 pr. ct. of the quantity taken. Cobaltous carbonate may be treated in the same manner, but the alkali cannot be completely washed out, and the method is in this case not to be recommended.

It seems at least extremely probable that other gelatinous oxyds and hydrates will give equally good results when treated in the manner which I have described.

§ 4.

On the precipitation of antimonous sulphid from boiling solutions; by Stephen P. Sharples.

In the precipitation of antimonous sulphid I have found it of very great advantage to employ the following process: Into the solution, containing as usual tartaric and free chlorhydric acid, a current of sulphydric acid gas is to be passed, the liquid being, during the passage of the gas, gradually heated to the boiling point. The boiling is then to be continued for 15 or 20 minutes, the current of gas passing uninterruptedly, until the voluminous sulphid has become a dense granular powder, occupying but a small portion of the original volume of the sulphid. The sulphid may then be washed with great facility, and dried upon a sand filter at 200°–300° C. All the determinations of antimony made in this Laboratory for some years have been executed in this manner, the results leaving nothing

to be desired. Arsenous sulphid does not become granular and dense under the same circumstances. In this connection I may be permitted to mention that the sulphids of nickel and cobalt, when precipitated from boiling solutions in the manner recommended by Prof. Gibbs some years since, should be filtered off, and washed immediately after precipitation. In this manner there is no oxydation upon the filter, even during the drying of the precipitate. But if the sulphids are allowed to stand in the solution from which they have been precipitated, for even a few hours, they will usually oxydize upon the filter during the washing.

§ 5.

On the introduction of the principle of repetition into chemical analysis; by BRYANT GODWIN.

The method of repetition, so frequently and so advantageously employed in physical investigations, has not, so far as I am aware, been applied to chemical analysis. It seems at least desirable that it should be so applied, and I will here give a particular instance in which it may be employed with advantage. In the determination of iron by means of potassic hypermanganate, all the iron is, at the end of the operation, in the form of sesquioxyd, while there is also a very small excess of unreduced hypermanganate. When the solution is boiled for a short time with pure zinc-dust and then filtered through a ribbed filter, which is quickly washed with water previously boiled to expel air, the iron is wholly in the form of ferrous oxyd, and the process of titration may be repeated a second time. After a new reduction the iron may again be determined, and this process may be repeated until the volume of liquid becomes too large to be easily handled. The following analyses were made to test this process:

0·4725 gr. ammonio-ferrous sulphate required in 5 successive titrations 49·0, 47·2, 48·7, 48·0, 48·5 cubic centimeters of potassic hypermanganate, 1 c.c. corresponding to 0·0014 gr. iron. The mean of these 5 determinations gives 14·31 pr. ct. iron in the salt.

0·4888 gr. required in 7 successive titrations 49·5, 48·75, 50·5, 49·8, 49·7, 49·8, 49·5 c.c. of hypermanganate, the mean of which gives 14·23 pr. ct. iron.

The formula requires 14·27 pr. ct.

These analyses, which with more practice and experience on my part would doubtless have corresponded much more closely, will at least serve to show that the principle of repeated observations of the same quantity to be measured may sometimes be introduced into chemical analysis.

Art. XXVI.—*On the Corona seen in Total Eclipses of the Sun;* by Professor W. A. Norton.

Certain observations made on the total eclipse of the sun of August 7, 1869, have led some of the observers to the conclusion that the corona seen on that occasion, and in previous eclipses, is of the nature of a *Solar Aurora.* It is proper that it should be publicly stated that this theory is not a new one. It has been advocated for several years by the author of the present communication, both in publications and in public lectures. It is essentially involved in the explanation of the Zodiacal Light, propounded in his Treatise on Astronomy, 2nd edit. (1845); and in the theoretical views set forth in a note appended to the discussion of the topic of Terrestrial Magnetism, in a memoir on Molecular Physics, published in this Journal (1864–6).* It is distinctly presented in the last edition of the Treatise on Astronomy (1867), pp. 172, 174, 175, and 178. I propose, in a communication to the next No. of this Journal, to state the principal grounds upon which I have maintained the auroral origin of the corona in different publications, as well as give the results of my own observations on the eclipse of 1869, and of those of other observers of that and previous total eclipses, which lend a powerful support to the auroral theory of the corona.

This introductory notice is now published mainly with the view of calling, at an early day, the attention of astronomers who may observe the eclipse of December next, to the importance of noting the exact positions, with respect to the plane of the sun's equator, of the more prominent portions of the corona. From two to four points of special outstreaming have been observed in different eclipses. In the eclipse of last year the more conspicuous extensions of the corona were nearly in the plane of the sun's equator, and from the vicinity of the poles. The figure of the corona, accompanying the Report of P. Prof. Capelotti of observations on the eclipse of April 15, 1865, made at Chili, shows the same to have been the case in that eclipse. The delineations of the corona as seen in other eclipses, so far as I have been able to ascertain, fail to give any accurate indication of the positions of the more prominent parts from the absence of all lines of reference in the drawings. It is to be hoped that observers of subsequent eclipses will take the precaution to ascertain these positions, and note them in their Reports. If they really have any general uniformity, in different eclipses, the fact cannot fail to throw light on the origin and nature of the corona.

* See this Journal, vol. xli, No. 121, pp. 76, 77.

Art. XXVII.—*Observations on Prehistoric Archæology in Greece;* by George Finlay, LL.D.*

A few objects belonging to the stone period were observed in Greece before it was known that they are relics of the people who inhabited the country in prehistoric times. The pieces of obsidian, generally called flint by travelers, that were picked up on the tumulus of Marathon, were termed Persian arrow-heads. This arose from a strange misapplication of the mention of stone arrow-heads having been employed by the Ethiopians in the army of Xerxes by Herodotus (Polymnia, vii, 69), who says they used short arrows of reeds pointed with a stone with which they engraved their signets. But why Datis, who can hardly have had any Ethiopians in his army, thought it expedient to bring to Marathon immense quantities of stone arrow-heads has not been explained. They do not appear to have been likely to prove efficient missiles against Athenian hoplites. Yet a sagacious traveler like Dodwell, in 1805, says that he found "a great many small arrow-heads of black flint, which probably belonged to the Persian army."† Even Colonel Leske, the ablest and most observant of modern travelers, was misled by this opinion. He says, "while I was employed on the summit of the *Soros*, as the tumulus of the Athenians is called, my servant amused himself in gathering at the foot of the barrow, a great number of pieces of black flint which happened to strike his observation. These flints are so numerous, and have been so evidently chipped by art into their present form like gun flints, that there is good reason for believing them to have been the heads of arrows discharged by the Persians who fought at Marathon, and to have been interred with the Athenians after having been gathered from every part of the plain after the battle. Herodotus shows that some of the barbarians were armed in this manner, though his remark is applied not to the army of Darius but to that of Xerxes. Flint of this kind, if produced in any of the adjacent parts of Greece is at least very rare." (*Travels in Northern Greece*, vol. ii, p. 431). The great quantity and small size of the fragments found in the tumulus of Marathon caused the writer of these pages to doubt the possibility of these fragments having anything to do with the Persians, for such feeble weapons as they could form must have been useless against the panoply of the Greek infantry of the period. Had they been employed the glory of Marathon would be a vain boast. Sir William

* From a letter to Mr. H. T. DeForest, dated Athens, Feb. 26, 1870, communicated by him for this Journal.

† A classical and topographical tour through Greece during the years 1801, 1805 and 1806, by Edward Dodwell, Esq., vol. ii, p. 159.

Gell, in his *Itinerary of Greece*, page 166, mentions that similar fragments of flint are found at the σχίστη ὁδὸς, where was the tomb of Laius, and he adds "perhaps a confirmation of the discomfiture of the barbarians in the *Odos schiste*." These fragments of obsidian, wherever they are found in Greece, are now admitted to be relics of prehistoric times, and a careful examination of the tumulus of Marathon convinced the writer of these pages, as early as the year 1835, that they were scattered about in the soil in their actual state when it was heaped up to form the tumulus over the bodies of the Athenians who were slain in the battle. The material is obsidian from the island of Melos. * * * *

No traditions of the existence of a stone period appear to have reached the inhabitants of Greece in historic times, though the mythical history of the remains of Tyrinths and Lykosura ascend almost to the prehistoric ages. As I have already mentioned, my attention was first called to the certainty that a numerous race of people in Greece used stone implements by the fragments of obsidian picked up on the tumulus at Marathon. I subsequently observed that similar fragments of obsidian are found in various parts of the neighborhood in the rear of the Greek position, and far out of reach of the arrows of the Persians. I also found myself similar chips of obsidian over all Attica, and in many parts of Greece, and several of the islands of the Archipelago, where no native obsidian can ever have existed, which I visited after my attention was directed to the subject. I have picked up these so-called Persian arrow-heads even in the now barren island of Hydra. My first notice on the subject was published in the year 1836. In that year, while examining the topography of Attica I discovered the extensive deposit of fossil bones at Pikermi, of which there is a valuable collection in the Museum of Natural History at Athens. A detailed description of these remarkable fossil bones was published by Professor Roth of Munich, in the Transactions of the Royal Academy of Bavaria, and they have since been described in the splendid work of Monsieur A. Gaudry, *Animaux fossiles et Geologie de l'Attique*. In a notice of the discovery which I read at a meeting of the Society of Natural History of Athens on 13th December, 1836, I observed that I had picked up fragments of obsidian, called Persian arrow-heads, not only on the tumulus of Marathon but also at Liosia near Aphidna, at Kakosialesi near Tanagra, and at Aghios Kosmas on the Attic coast. When my memoir on the battle of Marathon, which was read to the Royal Society of Literature in January, 1838, was printed, I added a note "concerning the pieces of flint called Persian arrow-heads found in the tumulus at Marathon." (Transactions of the Royal Society of Literature, first series, 4to, vol. iii, p. 392).

I have since collected several specimens of stone implements, particularly celts, that is, axes and chisels very similar in form to those found in the lake dwellings of Switzerland. I have obtained six specimens of jade, one of which I have presented to the Museum, with a similar piece which I procured at Robenhausen. I have also a fine small axe of nephrite, and several others of extremely hard stones. Unfortunately it has not yet been in my power to ascertain the precise mineralogical character of the most remarkable of my specimens.

I have obtained several fine specimens from Dobrena (the site of the ancient Thisbe) where a lake must have existed in prehistoric times, and where there is still a marsh. Two small axes found at Orchomenas, near the lake Copaïs, were given to me by Mr. Merlin, Her Majesty's Consul for Northern Greece.

Lake dwellings continued to exist in Macedonia down to the time of Herodotus. His description (Terpsichore, v, 16) proves that the dwellings of the Pæonians, in the Lake Prasias, were very similar to those constructed on the lakes of Switzerland. "They who dwell on the lake Prasias construct their dwellings in this manner. They fix strong piles in the lake, and on these piles they fasten planks, making a bridge with a narrow entrance from the land. The piles supporting the planks were in former times fixed by the inhabitants in common, but afterward the law established that every one who married a wife (and they take many wives), should bring down from Mount Orbelos three piles and fix them in the lake. The manner of their dwellings is in this fashion. Each man has his own hut on the piles, and a trap door through the flooring by which he can descend to the lake. The young children are tied by the foot lest they should fall into the water." The lake Prasias is the lake of the Strymon of Thucydides, v, 7, the Kerkinites of Arrian, Anab. 1, 11, 3, and is now called Tachynós, from a village on its western side. The fisheries are still valuable as they were in ancient times, and the fish caught for sale are principally carp, tench and eels. (Leake, Travels in Northern Greece, vol. iii, 198).

The description which Herodotus gives of these lake dwellings makes it an object of the greatest importance to the archæologists of Switzerland and Greece that the lake Prasias and other lakes in Macedonia, Thessaly and Greece, should be carefully examined in order to ascertain whether any traces can still be discovered of lake dwellings. Some traces of the piles on which dwellings were constructed are said to have been observed in the lake Prasias, in 1862, by Monsieur Deville of *l'École française d'Athénes.* But a superficial examination might easily lead to considering stakes for nets or fishing huts

as remains of ancient pile dwellings, and a searching examination of the locality ought to be undertaken by skillful and experienced observers.

The lakes in Greece that deserve particular attention are, the lake Copais, Hylica or Livadi, and Paralimni in Bœotia, the lake Trichonis with its connected lake Hyrie in Etalia, the lakes in Acarnania and the lagoons between the mouths of the Evenus and the Achelous. In the Peloponnesus there are, the lakes of Pheneus (which becomes in alternate periods of years a deep lake as at present, and a plain that dries in summers as it was in the year 1821), and Stymphalus, with their physical peculiarities and mythical associations running back toward a prehistoric period. The lake of Orchomenos, the marsh of Mantinea, the lakes near Tegea, the lagoons at the mouths of the Eurotas and the Alpheus, and the marsh at Pylos, (Palæo Avarino) all these places, and some others that might be pointed out, offer an extensive field for research. It may also be possible to identify sites of prehistoric habitations in the mountains, from the remains found in their vicinity. Such positions would have been selected because they were easily defensible by men having weapons of stone only. They must have commanded access to an abundant supply of water equally capable of defense. I have observed such a position overlooking the plain of Aphidna, where I have picked up a considerable quantity of fragments of obsidian and flint artificially worked.

Pliny contains several passages in his Natural History that refer to stone axes and chisels (celts), with particular reference to those found in Greece, for he quotes Greek authorities about them. He speaks of *Ceraunicæ* (thunderbolts) as being, according to the testimony of Sotacus (an ancient Greek writer on minerals), black and red, and resembling axe-heads in shape. I have specimens of red celts from Eubœa made of red ironstone, one $3\frac{3}{4}$ in. long and 2 in. broad; and several that are black, of the same size but narrower.

The stone period has been divided into a paleolithic and a neolithic period. In western Europe, particularly in France and England, numerous remains of stone implements of the paleolithic period have been found in strata with the bones of the mammoth and other extinct animals. But I am not aware that any stone implements that can be attributed with any certainty to this period, have yet been discovered in Greece, though bones of these animals have been found in several places in great quantities both in Attica, Eubœa and Arcadia. All the stone implements that have fallen under my notice consist of specimens that belong to the neolithic or polished-stone period, and many display considerable skill in their workmanship, being composed of the hardest stones.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the theory of the Bunsen flame.*—The non-luminosity of the flame of the Bunsen gas-burner is commonly ascribed to the more complete combustion of the gas by the air which is mixed with it before it is burned. But this is an assumption entirely unproved as yet; and moreover, the positive experiments of KNAPP show that this cannot be the only cause of the phenomenon. His burner was so constructed that by means of a lateral tube, other gases beside air could be mixed with the coal-gas; and the results obtained prove that nitrogen, hydrochloric acid, or carbon dioxyd gas causes the flame to burn fully as blue as is seen in the burner as ordinarily used. The experiment is most easily tried with carbon dioxyd gas; but with this gas it might be urged that dissociation into carbonous oxyd and oxygen takes place, and that the latter gas assists the combustion. Though this is hardly conceivable at the low temperature there present, the assumption may be entirely disproved by using in the experiment only pure nitrogen; which for this purpose may be conveniently obtained by boiling a solution containing equivalent quantities of potassium nitrite and ammonium chlorid. The explanation of this result is not easy; Knapp believes that the disappearance of the luminosity is due partly to the cooling of the flame, but principally to the dilution of the illuminating gas; and that the flame of the Bunsen burner is non-luminous from the same cause which lessens the light of a candle burned in vacuo, or at high altitudes.—*J. Pr. Ch.*, II, i, 428, June, 1870. G. F. B.

2. *Production of Ozone in Rapid Combustion.*—The observation of Loew upon the production of ozone in quick combustion, published in the May number of this Journal, seems to have been anticipated not only by Pincus (noticed in vol. xlvii, page 238,) but much earlier—in December, 1864—by THAN, and communicated to the Hungarian Academy at Pesth. Becoming interested in the application to vegetable physiology of the fact that ammonium nitrite is formed when hydrogen burns in an atmosphere containing nitrogen, Than sought for a method of exhibiting the result as a lecture-experiment; for this purpose he drew the air which closely surrounds the flame of a Bunsen burner through a Varrentrapp and Will's bulb-apparatus, containing an acidulated solution of potassium iodid and starch. The air which was drawn from about the lower $\frac{2}{3}$ of the flame, colored the solution blue very rapidly. As the odor of ozone was not detectable in this air after it had passed through the apparatus, Than substituted a bulb-tube filled with water; and then on drawing through this a rapid current of the air from this part of the flame, the odor of ozone was distinctly obtained. Were ammonium nitrite formed at the same time, it would be retained by the water; and this, on adding a

few drops of the acidulated potassium iodid solution, would become at once blue. But no blueing took place, even when the air was passed through the water for 15 or 20 minutes. The blueing in the former case must be due, therefore, entirely to ozone. Than next investigated the conditions of this ozone-production. He found that a small quantity of ozone is present in the air which surrounds the lower portion of every flame composed of burning hydrogen; that this ozone may be detected by withdrawing this air rapidly from the flame through a glass tube drawn out to a fine point, when it gives its characteristic odor and reactions; that the point of the tube should not exceed a millimeter in diameter, and that it should be placed in the lower half of the flame where the flame comes in contact with the cold air. In a candle-flame, or that of an alcohol-lamp, most ozone is found near the lower blue portion. The current of air must be rapid enough to deflect the flame, yet not so rapid as to draw in any unburned gas. By the ozone thus obtained, the solution of potassium iodid and starch was blued in a few seconds, a mixture of manganous sulphate solution with a few drops of potassium hydrate became dark brown from the formation of the peroxyd, and when inhaled, the mucous membrane was irritated and a transient catarrh produced. No ozone could be detected in the air surrounding ignited charcoal; and all attempts to determine it in the other cases quantitatively, failed. Than then goes on to say: "Wenn man durch das obere Drittheil einer nicht leuchtenden Gasflamme, durch ein schief aufwärts gerichtete zwei Linien weite Glasröhre, einen sehr kräftigen Luftstrom mittelst eines Blasebalges durchtreibt, so kann man in der durchgeblasenen Luft nicht unbedeutende Mengen von Ozon theils durch den Geruch, theils durch Jodkalium-Stärkepapier nachweisen;" an experiment almost precisely that mentioned by Loew. In Than's view, the diatomic molecule of oxygen in the air, when it comes in contact with the hydrogen of the flame, gives up one atom, to form water, thus: $H_2+O_2=H_2O+O$; the other atom thus set free, unites with another molecule of oxygen to form ozone: $O_2+O=O_3$. No ozone is formed in the vicinity of the burning carbon, because each of its atoms unites directly with an entire molecule of oxygen, $C+O_2=CO_2$, producing carbon dioxyd.—*J. Pr. Ch.*, II, i, 415, June, 1870. G. F. B.

3. *On the Constitution of Narcotine and its decomposition-products.*—MATTHIESSEN thus sums up the results of his extended research upon Narcotine:—

(1.) The analysis of various specimens of narcotine, derived from various sources, has shown that it always has the same composition, $C_{22}H_{23}NO_7$.

(2.) As already noticed by previous observers, narcotine, by the action of oxydizing agents, splits up into opianic acid and cotarnin:

$$C_{22}H_{23}NO_7+O=C_{10}H_{10}O_5+C_{12}H_{13}NO_3.$$

(3.) Heated alone, to a temperature somewhat above 200°, or for

a somewhat longer time with water, narcotine splits up into meconin and cotarnin:

$$C_{22}H_{23}NO_7 = C_{10}H_{10}O_4 + C_{12}H_{13}NO_3.$$

(4.) Heated for a short time (about two hours) with an excess of hydrochloric acid, methyl chlorid is formed, and one atom of hydrogen replaces the methyl in the narcotine; by a longer heating (for some days) two atoms of H may replace two of CH_3; heated with fuming hydriodic acid, three atoms of the methyl are replaced by three of hydrogen; thus forming a series of homologous bases, whose decompositions are analogous to those of narcotine itself.

(5.) The formula of cotarnin has been shown to be $C_{12}H_{13}NO_3$, and not $C_{13}H_{13}NO_3$; it is capable of crystallizing with one-half and with a whole molecule of water.

(6.) Cotarnin, when heated with dilute nitric acid under certain, not yet clearly-defined conditions, is decomposed, yielding cotarnic acid and methylamine:

$$C_{12}H_{13}NO_3 + 2H_2O = C_{11}H_{12}O_5 + CH_5N.$$

By the use of concentrated nitric acid, as already noticed by former observers, apophyllic acid is formed. The action of other oxydizing agents gives results not yet studied.

(7.) Cotarnin heated with concentrated hydrochloric acid, yields methyl chlorid and the hydrochlorate of cotarnamic acid:

$$C_{12}H_{13}NO_3 + H_2O + 2HCl = CH_3Cl + C_{11}H_{13}NO_4, HCl.$$

Hydriodic acid produces a similar result; only one atom of CH_3 being eliminated from each molecule of cotarnin.

(8.) Opianic acid, by the action of nascent hydrogen (as by treatment with sodium-amalgam or by zinc and sulphuric acid) is reduced to meconin:

$$C_{10}H_{10}O_5 + H_2 = H_2O + C_{10}H_{10}O_4.$$

(9.) Opianic acid by heating with potassium dichromate and dilute sulphuric acid is oxydized to hemipinic acid:

$$C_{10}H_{10}O_5 + O = C_{10}H_{10}O_6.$$

(10.) Opianic acid, on heating with caustic potash, splits up into meconin and hemipinic acid:

$$2C_{10}H_{10}O_5 = C_{10}H_{10}O_4 + C_{10}H_{10}O_6.$$

(11.) Opianic acid, heated with an excess of hydrochloric acid, exchanges its methyl for hydrogen. Beside methyl chlorid, two substances are probably thus produced, noropianic acid and methyl-noropianic acid; the first by a single, the second by two replacements of methyl by hydrogen.

$$C_{10}H_{10}O_5 + 2HCl = 2CH_3Cl + C_8H_6O_5.$$
$$C_{10}H_{10}O_5 + HCl = CH_3Cl + C_9H_8O_5.$$

Only the latter has been obtained pure, the former spontaneously decomposing. Hydriodic acid acts similarly. Methyl-noropianic acid, like opianic acid, is monobasic.

(12.) All attempts to oxydize meconin to opianic or hemipinic acid, or to any other product, were unsuccessful.

(13.) Meconin, treated with an excess of hydrochloric or hydriodic acid, yields methyl chlorid or iodid, and a body derived from meconin by replacing $\mathrm{CH_3}$ by H, methyl-normeconin:

$$\mathrm{C_{10}H_{10}O_4 + HCl = CH_3Cl + C_9H_8O_4}.$$

Experiments to produce the hypothetical normeconin by substituting $\mathrm{H_2}$ for $(\mathrm{CH_3})_2$, gave nothing capable of being isolated in a pure state.

(14.) Hemipinic acid, by treatment with various reducing agents, was in no case reduced to opianic acid or meconin. So, experiments to obtain opianic acid from the union of hemipinic acid and meconin, have yielded no result. Moreover, hemipinic acid could not be oxydized to any other product.

(15.) Heated with an excess of hydrochloric acid, hemipinic acid yields, beside methyl chlorid and carbon dioxyd, a new acid, methyl-hypogallic acid:

$$\mathrm{C_{10}H_{10}O_6 + HCl = CH_3Cl + CO_2 + C_8H_8O_4}.$$

Heated with hydriodic acid, it affords methyl iodid, carbon dioxyd and hypo-gallic acid:

$$\mathrm{C_{10}H_{10}O_6 + 2HI = 2CH_3I + CO_2 + C_7H_6O_4}.$$

(16.) Anderson's observations proving hemipinic acid to be dibasic are confirmed, the anhydrid being obtained by simple drying:

$$\mathrm{C_{10}H_{10}O_6 = H_2O + C_{10}H_8O_6}.$$

Methyl-hypogallic acid is also dibasic.

(17.) Hemipinic acid may crystallize with various quantities of crystal water; crystals having been obtained with $\frac{1}{2}$, with 1, and with 2 molecules of such water.

(18.) All the reactions of narcotine and of its decomposition products, are explained in a satisfactory manner by assuming for it the following rational formula:

$$\left.\begin{matrix} \mathrm{CH_3} \\ (\mathrm{C_{11}H_9O_2})'' \end{matrix}\right\} \mathrm{N}$$

$$\left.\begin{matrix} (\mathrm{C_8H_4O})^{\mathrm{iv}} \end{matrix}\right\} \mathrm{O_3}$$

$$\left.\begin{matrix} (\mathrm{CH_3})_2\mathrm{H} \end{matrix}\right\} \mathrm{O_3}$$

—*Ann. Ch. Pharm.*, Suppl. Band vii, 66, Nov., 1869. G. F. B.

4. *On the size of Molecules;* by Sir Wm. Thomson. (Proc. Lit. and Phil. Soc. Manchester, March 22; Nature, May 19, p. 56.)—My occupation on the Kinetic theory of gases has led me at last to come to definite terms as to the size of molecules. Ever since about the first year of my professorship I have taught my students that Cauchy's theory of Dispersion proves heterogeneousness, or molecular structure, to become sensible in contiguous portions of glass or water, of dimensions moderately small in comparison with the wave-lengths of ordinary light. I have spoken to you also, I think, of the argument deducible from the contact electricity of metals. This, I now find, proves a limit to the dimensions of the

molecules in metals quite corresponding to that established for transparent solids and liquids by the dynamics of dispersion. In experiments made about ten years ago, of which a slight sketch is published in the Proceedings of the Literary and Philosophical Society of Manchester, I found that a plate of zinc and a plate of copper kept in metallic connection with one another (by a fine wire or otherwise) act electrically upon electrified bodies in their neighborhood, and upon one another, as they would if they were of the same metal and kept at a difference of potentials equal to about three-quarters of that produced by a single cell of Daniell's. Hence, and from my measurement of the electrostatic effects of a Daniell's battery, published in the Proceedings of the Royal Society, for February and April, 1860, I find that plates of zinc and copper held parallel to one another at any distance, D, apart which is a small fraction of the linear dimensions of their opposed surfaces, and kept in metallic communication with one another, exercise a mutual attraction equal to

$$2\times10^{-10}\times\frac{A}{D^2}\text{ grams weight.}$$

Hence, if they were allowed to approach from any greater distance, D′, to the distance, D, the work done by their mutual attraction is

$$2\times10^{-10}\times\frac{A(D'-D)}{D'D}\text{ centimeter grams;}$$

which, if D is very small in comparison with D′, is very approximately equal to $2\times10^{-10}\times\frac{A}{D}$.

Now suppose a pile to be made of a great number (N+1) of very thin plates alternately of zinc and copper, kept in metallic connection while they are brought toward one another. Let their positions in the pile be parallel, with narrow spaces intervening. For simplicity, let the thickness of each metal plate and intervening space be D. The whole work done will be

$$2\times10^{-10}\times N\frac{A}{D}.$$

The whole mass of the pile (if we neglect that of one of the end plates) is $NAD\varrho$, where ϱ denotes the mean of the densities of zinc and copper. Hence, if h be the height to which the whole mass must be raised against a constant force equal to its weight at the earth's surface, to do the same amount of work, we have

$$NAD\varrho h=2\times10^{-10}\times N\frac{A}{D};$$

which gives $$h=\frac{2\times10^{-10}}{\varrho D},$$

or, as $\varrho=8$, nearly enough for the present rough estimate,

$$h=\frac{1}{(200000D)^2}.$$

Hence, if $D=\frac{1}{200000}$ centimeter,
$h=1$ centimeter.

The amount of energy thus calculated is not so great as to afford any argument against the conclusion which general knowledge of divisibility, electric conductivity, and other properties of matter indicates as probable; that, down to thicknesses of $\frac{1}{200000}$ of a centimeter for the metal plates and intervening spaces, the contact electrification, and the attraction due to it, follow with but little if any sensible deviation the laws proved by experiment for plates of measurable thickness with measurable intervals between them. But let D be a two-hundred-millionth of a centimeter. If the preceding formulæ were applicable to plates and spaces of this degree of thinness we should have

$$h=1{,}000{,}000 \text{ centimeters or } 10 \text{ kilometers.}$$

The thermal equivalent of the work thus represented is about 248 times the quantity of heat required to warm the whole mass (composed of equal masses of zinc and copper) by 1° Cent. This is probably much more than the whole heat of combination of equal masses of zinc and copper melted together. For it is not probable that the compound metal when dissolved in an acid would show anything approaching to so great a deficiency in the heat evolved below that evolved when the metallic constituents are separately dissolved, and their solutions mixed; but the experiment should be made. Without any such experiment, however, we may safely say that the fourfold amount of energy indicated by the preceding formula, for a value of D yet twice as small, is very much greater than any estimate which our present knowledge allows us to accept for the heat of combination of zinc and copper. For something much less than the thermal equivalent of that amount of energy would melt the zinc and copper; and, therefore, if in combining they generated by their mutual attraction any such amount of energy, a mixture of zinc and copper filings would rush into combination (as the ingredients of gunpowder do) on being heated enough in any small part of the whole mass to melt together there. Hence, we may infer that the electric attraction between metallically-connected plates of zinc and copper of only $\frac{1}{4000000000}$ of a centimeter thickness, at a distance of only $\frac{1}{4000000000}$ of a centimeter asunder, must be greatly less than that calculated from the magnitude of the force and the law of its variation observed for places of measurable thickness, at measurable distances asunder. In other words, plates of zinc and copper so thin as a four-hundred-millionth of a centimeter from one another, form a mixture closely approaching to a molecular combination, if, indeed, plates so thin could be made without splitting atoms. Wishing to avoid complication, I have avoided hitherto noticing one important question as to the energy concerned in the electric attraction of metallically-connected plates of zinc and copper. Is there not a change of temperature in molecularly thin strata of the two metals adjoining to the opposed surfaces, when they are allowed to approach one another, analogous to the heat produced by the condensation of a

gas, the changes of temperature produced by the application of stresses to elastic solids which you have investigated experimentally, and the cooling effect I have proved to be produced by drawing out a liquid film which I shall have to notice particularly below? Easy enough experiments on the contact electricity of metals will answer this question. If the contact-difference diminishes as the temperature is raised, it will follow from the Second Law of Thermodynamics, by reasoning precisely corresponding with that which I applied to the liquid film in my letters to you of February 2d and February 3d, 1858,* that plates of the two metals kept in metallic communication and allowed to approach one another will experience an elevation of temperature. But if the contact-difference increases with temperature, the effect of mutual approach will be a lowering of temperature. On the former supposition, the diminution of intrinsic energy in quantities of zinc and copper, consequent on mutual approach with temperature kept constant, will be greater, and on the latter supposition less, than I have estimated above. Till the requisite experiments are made, further speculation on this subject is profitless; but whatever be the result, it cannot invalidate the conclusion that a stratum of $\frac{1}{2000000000}$ of a centimeter thick cannot contain in its thickness many, if so much as one, molecular constituent of the mass. Besides the two reasons for limiting the smallness of atoms or molecules which I have now stated, two others are afforded by the theory of capillary attraction, and Clausius' and Maxwell's magnificent working out of the Kinetic Theory of gases. In my letters to you already referred to, I showed that the dynamic value of the heat required to prevent a bubble from cooling when stretched is rather more than half the work spent in stretching it. Hence, if we calculate the work required to stretch it to any stated extent, and multiply the result by $\frac{3}{2}$, we have an estimate, near enough for my present purpose, of the augmentation of energy experienced by a liquid film when stretched and kept at a constant temperature. Taking ·08 of a gram weight per centimeter of breadth as the capillary tension of a surface of water, and therefore ·16 as that of a water bubble, I calculate (as you may verify easily) that a quantity of water extended to a thinness of $\frac{1}{2000000000}$ of a centimeter would, if its tension remained constant, have more energy than the same mass of water in ordinary condition by about 1,100 times as much as suffices to warm it by 1° Cent. This is more than enough (as Maxwell suggested to me) to drive the liquid into vapor. Hence, if a film of $\frac{1}{2000000000}$ of a centimeter thick can exist as liquid at all, it is *perfectly certain* that there cannot be many molecules in its thickness. The argument from the Kinetic Theory of gases leads me to quite a similar conclusion.

5. *Comparison of Mechanical Equivalents;* by PLINY EARLE CHASE, (Proc. Am. Phil. Soc., xi, 313, 1870).—The comparison of different mechanical equivalents will open a new field for investigation, which may prove to be fertile in valuable results. For

* Proceedings of the Royal Society for April, 1858.

example, recent determinations, by the different methods of Thomsen and Farmer, fix the mechanical equivalent of light, in a wax candle burning 126½ grains per hour, at 13·1 foot-pounds per minute, the equivalent of 1 grain being 6·213 foot-pounds. According to Dulong, the heat evolved during the combustion of 1 grain of olive oil in oxygen, is sufficient to heat 9862 grains of water 1° C. According to Favre and Silbermann, 1 grain of oil of turpentine, burned in oxygen, would heat 10,852 grains of water 1° C.

It may therefore be presumed that the total heat given out by the combustion of 1 grain of wax, is about sufficient to raise 10,000 grains or water 1° C., or 18,000 gr. 1° F. This represents a mechanical equivalence of $(18{,}000 \times 772 \div 7000 =)$ 1985·143 foot-pounds, which is 319·5 times as great as the corresponding equivalent of the light given out during the combustion.

Tyndall, in his lecture on Radiation, states that the visible rays of the electric light contain about one-tenth of the total radiated heat. The relative luminous intensity of an electric lamp would therefore appear to be about 32 times as great as that of the wax candle. This ratio so nearly resembles that of solar to terrestrial superficial attraction, and the connection of electric and magnetic currents with solar radiation is so evident, that additional experiments, to furnish materials for a great variety of similar comparisons, seem desirable. While it is possible that the resemblance, in the present instance, may be accidental, the numerous harmonies between the manifestations of cosmical and molecular forces render it at least equally possible that it may have a weighty significance.

II. GEOLOGY AND MINERALOGY.

1. *On a Fossil Tooth from Table Mountain;* by Prof. William P. Blake. (Communicated by the author for this Journal.)—The fossil tooth, found by Mr. D. T. Hughes, 1,700 feet under Table Mountain, and 300 feet below the surface, I have carefully examined and compared with specimens in the Smithsonian Institution. It proves to be a back lower molar of an equine animal of the genus *Hipparion*, or a closely allied genus. This genus is one of the connecting links between the *Palæotherium* and the horse.

The specimen closely resembles a fossil in the Smithsonian museum, from the Pliocene formations of the Niobrara river in Nebraska,* not only in size but in the foldings of the enamel, and particularly in the posterior part of the tooth, but it differs enough, in several particulars, to justify the belief that it is a distinct species. Dr. Leidy does not attempt to determine, specifically, the specimen from Nebraska, but considers it closely related to, if not identical specifically with, *Hipparion gratum*, possibly *Protohippus placidus*.

* Described by Prof. Leidy in his work upon the Extinct Mammalian Fauna of that region, p. 319, pl. xix, fig. 7,

The size of the Table Mountain specimen, which is considerably worn by attrition in the gravel, is: length, 11 lines; breadth upon the crown, 9 lines; breadth at the base, 10 lines; thickness, anteriorly, 4 lines, posteriorly, 2 lines.

This fossil is the first of the kind discovered west of the Rocky Mountains. It adds to the list of the fauna of the period antedating Table Mountain—a list which includes the mammoth (*Elephas*, from Knight's Ferry), the rhinoceros, and an animal allied to the elk. I have believed that remains of man were also found under the lava; but upon this point, after diligent inquiry, I am satisfied that the evidence is insufficient. But we now add this fossil allied to *Hipparion*, and I regard it as another indication that the Table Mountain beds are Pliocene, and homotaxial with those of the Bad Lands of Nebraska.

2. *Cause of the Descent of Glaciers.*—Rev. Henry Moseley, before the Royal Society in January, 1869, in the Philosophical Magazine for the May following, and at the meeting of the Royal Institution of Great Britain, May 13, 1870, opposes the view that glaciers owe their movement mainly to gravity, and gives as the principal cause contraction and expansion due to change of temperature through the mass. He compares the movement to that of a sheet of lead on a sloping surface, in its expansion the lower edge working downward, and in its contraction the upper edge or part.

In the Phil. Mag. for July, 1870, Mr. John Ball, after alluding to the criticisms on the above theory by Mr. Wm. Mathews in the Alpine Journal for February last, shows that the supposed contraction and expansion to which Canon Moseley appeals, does not take place, and that the "crawling theory" of glacier motion is therefore unsatisfactory. He argues that the glacier is not a continuous solid mass like a sheet of metal; that the temperature of the interior, as observers have proved, is very nearly constant; that the movement is half as fast in winter as in summer; that the rate of motion is not proportioned at all to the length of the glacier as it should be by the theory; that the supposed expansion and contraction, if a fact, would exceed twenty or more times the rate of actual motion but for modifying causes,—and this is an amount of modifying intervention for the sake of the theory, sufficient to prove the theory of no value. He concludes as follows:

"If I might presume to estimate the net results of this renewed discussion of the causes of glacier-motion, I should say that they are not considerable, but yet are far from worthless. Canon Moseley's experiments have added something to our knowledge, and especially those on the tenacity of ice, which have some bearing on the origin of crevasses. Of far greater importance are the observations on ice-planks made by Mr. William Mathews. The first of these, published in the 'Alpine Journal,' gave prominence to a fact which had long been familiar to myself, and probably to many others. I have often found that long icicles placed in an inclined position, and supported only at the upper end, will gradually resume the vertical direction, and I had, perhaps too lightly, assumed that this was a particular instance of the process by

which ice changes its form through fracture and regelation. In Mr. Mathew's first experiment, conducted during a thaw, a thick plank of ice supported at each end was deflected at the middle through a space of 7 inches in as many hours. Although none but very minute fissures were observed, the facts did not seem to me altogether inconsistent with that explanation. In the second series of observations, made during the severe frost of February last, Mr. Mathews found that at temperatures notably below the freezing-point a plank of ice, supported as before, subsides slowly between the points of support under the sole influence of its own weight. The deflection under these circumstances was about 1½ inch in twenty-four hours. Taking this observation in connection with a multitude of facts recently brought to light, and especially the researches of M. Tresca, we are led to admit that ice, in common with very many apparently rigid bodies, does possess a certain degree of plasticity which is exhibited by changes of form effected very slowly under the action of forces of moderate amount, rather than by the rapid action of more powerful agencies.*

"The admission of this conclusion may slightly modify, but will not materially alter, the views now generally held as to the causes of glacier-motion, which are mainly derived from the remarkable researches of Professor Tyndall. Whatever may be the final judgment of men of science, I feel quite sure that it will not confirm the opinion expressed by Canon Moseley in his latest publication: that "the phenomena of glacier-motion belong rather to mechanical philosophy than to physics." Every real advance that has been made toward the explanation of those phenomena has been due to the application of increased knowledge of the physical properties of glacier-ice; and if any thing be wanting to complete the explanation now generally accepted, it must be derived from such additional acquaintance with those properties as may be derived from continued observation and experiment."

3. *The North American Lakes considered as Chronometers of Post-glacial time;* by Dr. EDMUND ANDREWS. 24 pp. roy. 8vo. (Trans. Acad. Sci. Chicago, vol. II).—Dr. Andrews discusses in this paper the nature of the post-glacial deposits on the shores of Lake Michigan, especially in the vicinity of Chicago, their extent, the areas of the several beaches, the erosion these beaches have undergone, the width of the subaqueous plateau formed along the border of the lake out of the material removed in the erosion, and the amount of sand moved in the process; and from the elements thus obtained, arrives at the following conclusions:

(1.) The upper beach began to form immediately after the Boulder Drift period, and continued to accrete for about 900 years. No animal fossils have yet been found in it.

(2.) The waters then fell suddenly to about their present level, where they remained till a thin bed of peat accreted on the marshy slope vacated by the waves. I have not been able to collect data for a calculation of this first low-water period, but from the posi-

* In Dana's Manual of Geology, p. 673, this plasticity is recognized among the means of motion on the ground of observations, similar to the above, made by Kane in his "Arctic Explorations."

tion of the soil-bed in the eastern dunes, I incline to think it lasted 500 or 1000 years.

(3.) The water rose again, submerging for a short time the upper beach, but soon fell to the line of the middle one, where it remained about 1,600 or 2000 years. This period appears to be cotemporary with the Loess.

(4.) The water, which had already slowly fallen some feet, now retired more rapidly to near its present level, which it has maintained with only moderate fluctuations ever since.

(5.) The total time of all these deposits appears to be somewhere between 5,300 and 7,500 years.

The discussion is an interesting and important one. Some uncertainties in the calculations occur to us; but without a special examination of the region we are not at present prepared to mention any but the following. The author writes as if he supposed that sand in the course of transportation always remained sand. He observes that "the sand movement in the lake is confined to the shore line," as proved by the fact that "there is no sand in deep water," not recognizing the well-known geological fact that sands on coasts are always undergoing wear through the attrition of grain upon grain under the action of waves and currents, and that while the finer material made by this attrition is floated off to deeper waters, the coarser is left behind in such cases near or on the shores.

3. *Fossils in the Mineral veins of the Carboniferous Limestone of Great Britain.*—A paper on this subject by Mr. CHARLES MOORE, (Rep. Brit. Assoc. for 1869, p. 360), contains notices of numerous fossils in the Lead mines of the Carboniferous limestone. In the walls of the Charterhouse Lead-mine, in the Mendip range, 270 feet from the surface, over 80 species of Liassic fossils were obtained by him, and more than 30 of Carboniferous. The Liassic included a Chara, wood in the form of jet, Rhizopods, Pentacrinites, a Cidaris and other Echinoderms, Serpula, claws of Crustacea, many Mollusks, remains of about 10 species of fishes of the genera *Acrodus*, *Hybodus*, *Lepidotus*, &c., and a tooth of an Ichthyosaur; and among the Carboniferous, there were species of Helix, Hydrobia, Planorbis, Proserpina, Valvata, Vertigo, all either land or freshwater Mollusks, also 11 species of Ostracoids, besides Mollusks, Serpulæ, Encrinites, Corals and Conodonts. A similar range of facts was observed in connection with other lead mines. We cite the following general remarks.

Whilst the various mines and mineral deposits I have examined have certain species in common, it may be said that they have each special paleontological features of their own.

In the Keld-Head Mines organic remains are very abundant at about 450 feet from the surface, amongst which are many Foraminifera, chiefly of the genus *Involutina*, of which there are six species, and univalves of about twelve genera, the freshwater species *Valvata anomala* Moore, and *Planorbis Mendipensis*

Moore, being present, and also Entomostraca of several new species.

The Fallowfield mines, although not yielding a very long list of species, have their special interest in the presence of the land and freshwater genera *Stoastoma*?, *Hydrobia*, and *Pisidium*; *Involutina*, as in the Keld-Head mines, though rarely; and a single seed of the *Flemingites gracilis* Carr. The richest samples from this mine are at 90 and 450 feet from the surface.

The Grassington mines are not only very rich in individual specimens, but have yielded the greatest number of species, among which are again freshwater remains of *Hydrobia*, *Planorbis*, *Valvata*, and *Lithoglyphus*. Entomostraca of at least ten species, Conodonts of several varieties, and fish-remains of the genera *Petalodus*, *Orodus*, &c.

The Alston mines have yielded about twelve species of univalves, though they are not in good condition. Foraminifera are present, but are rare, and fish-remains of the genera *Petalodus*.

The Weardale mines, and those of Allenheads, are comparatively not rich, the vein stuff in them being much mineralized. Conodonts occur in the former, Entomostraca rather abundantly in the latter, and also, though rarely, the genus *Hydrobia*. In these veins, and also at Alston, I have detected, for the first time, large cells of a foraminiferous shell, for which Mr. Brady suggests the generic name *Carteria*.

In the White and Silver Band mines remains are somewhat rarely distributed, the richest deposit being a friable ochreous sandstone, on the "sun" side of the Silver Band Old Mine, which yielded many specimens of *Hydrobia*, and one or two of *Valvata anomala*, several genera of Foraminifera, including *Involutina* and *Dentalina*, with Conodonts, and portions of teeth of *Psammodus*.

The Mount-Pleasant mines of Mold contain Foraminifera, and also the freshwater *Hydrobia*, though rarely, and Conodonts rather abundantly; but they are especially remarkable for the great variety of fish-remains they yield, which appear to represent at least ten different genera. Mixed with the "dowks" of the mine are occasionally small pieces of laminated stone the surfaces of which exhibit numerous traces of fish-scales.

The researches I have been making have involved very considerable labor and minute investigation; but as they will to some extent have opened up a new field of inquiry, I hope they will not be without some results. Before concluding, I desire to refer to several of the more interesting paleontological facts which have been obtained. * * * * * *

Not the least important fact in my mine explorations has been the discovery of a land and freshwater fauna. Until I obtained the three genera of *Helix*, *Vertigo*, and *Proserpina*, with the freshwater genera *Planorbis* and *Valvata*, in the Charterhouse Mine, the only known terrestrial shell below the secondary beds was the *Pupa vestusta* Daws., found by Sir Charles Lyell and Dr.

Dawson in the Coal measures of Nova Scotia. To the above genera I have now to add those of *Hydrobia*, *Stoastoma ?*, *Lithoglyphus*, and *Pisidium*, from the mines of the north of England, some of which I have little doubt are older than the *Pupa vetusta* of the coal-beds. There is thus the fact of the presence of nine genera of land and freshwater shells in the lead-veins of this country.

In addition to the list of organic remains which follows, numbering about 112 species from the north of England and North-Wales mines, eight, which are not in common, have been obtained from Weston, and to these again are to be added 89 in the list previously given from Charterhouse, so that in true and workable mineral veins I have found 209 species. In the Carboniferous Limestone of the Frome district precisely similar phenomena occur, though the fissures are not worked. These Rhætic and Liassic veins have yielded me about 70 species, so that, including the districts I have enumerated, I have obtained from vein-fissures, with their deposits of different ages, about 279 species of organic remains.

Under these peculiar circumstances, I have discovered the oldest known Mammalia, the oldest land and freshwater Mollusca, about 52 species of fish, and about 8 of Reptilia, besides the other groups to which reference has been made.

With regard to the origin of the veins, Mr. Moore observes as follows:

The chief material of all the mineral veins I find to be of marine origin; all the organic contents are fossil, and their precise geological age can be arrived at without much difficulty. Wherever they contain land shells, as on the Mendips, or freshwater shells, which occur in the veins of Alston, and are wide-spread elsewhere, they are also fossil and of contemporaneous age with the other remains. It is certain from this that the veins received their infilling when within the influence of the ocean, and before their present elevation, since which time, as I have before stated, I doubt if there could be any material alteration in their contents. * * *

It has now been established, without doubt, by the highest chemical authorities, that many of our most important minerals are present in minute quantities in the waters of the ocean. This is admitted by those who believe in segregation, the difference being that they think it was first deposited and afterward extracted from the parent rock, and redeposited in the veins, rather than originally collected in the veins themselves.

As regards the connection subsisting between the ocean and the vein-fissures, I believe it will be recognized to be the case that, in the great majority of instances, the different veins come directly to the surface, and wherever a later rock has been deposited, which is only in exceptional cases, covering up the mouth of the vein, there will still be found a break in the sequence of the strata, which might give almost unlimited time for the precipitation of the minerals therein. Wherever systems of veins occur, it is prob-

able there will be found connected therewith an abnormal condition and considerable breaks in the deposition of the rocks. I have shown this to be the case in connection with the Carboniferous Limestone of the Mendip range, and its continuation through South Wales, in which districts it can be seen that those rocks were, through enormous periods, exposed to the influence of the ocean, possibly forming reef-like barriers around the edges of the Carboniferous basin, the fissured veins and floor of this sea-bottom receiving at some periods materials of Rhætic or of Lower or Middle Lias age, whilst an occasional capping of the beds of Inferior Oolite, left in some Carboniferous limestone trough, now and then cover up the mouths of the veins that had received all their contents prior to its deposition. Some most instructive examples are present in this district, in which it may be seen that whilst there are on the walls of the vein the usual vertical conditions of vein-stuff, such as calc-spar, sulphate of barytes, &c., with occasional hematite iron-ore, calamine, and galena, the central portion of the vein is unmistakably of Liassic or Rhætic age. In all such instances there are combined the elements of open fissures communicating with the ocean, and greater or less time in the reception of their contents.

4. *Additional note on Elasmosaurus;* by E. D. Cope.—To my preceding note on Elasmosaurus, I append the following, in consequence of the reading of another criticism by Prof. Leidy, in the Proceedings Acad. Nat. Sci. Philad., January to April, 1870, (issued in June). [See p. 139 of this volume.—Eds.].

In this Dr. Leidy agrees with my identification of Cimoliasaurus and Discosaurus made in 1868, regarding them as the same. But he employs the name Discosaurus instead of Cimoliasaurus, to which we object for three reasons: 1, in works written subsequent to his determination, Cimoliasaurus had been exclusively used, and has therefore obtained considerable currency; 2, Discosaurus was founded upon a miscellaneous collection of species, and not defined; 3, the name refers perhaps to an individual peculiarity of one of the species, as suggested by Leidy, and conveys an erroneous impression of there being a vertebral disc characteristic of the genus, whereas the peculiarity consists of a groove. The name Cimoliasaurus is open to none of these objections.

He however unites with the above genus my Elasmosaurus, although a few pages previously he considers them distinct, on the same grounds that convinced me of the propriety of separating them, viz: the enormous neck with compressed vertebræ in the one, and the short transverse cervicals of the other. No such difference is displayed by the species of Plesiosaurus, though there is considerable variation in the genus in this respect. It cannot however be predicted, that no species combining the characters of the two will ever be found. All genera in paleontology stand open to this risk.

The *Cimoliasaurus grandis* (*Brimosaurus* Leidy) presents the shortened cervicals of *C. magnus* and therefore is not an Elasmo-

saurus. The *E. orientalis* is as yet but little known, and there may be doubts as to which genus it represents. In a restored figure of it which was given in an article in the American Naturalist (1868, p. 84), it is represented with a neck of the shorter type of Cimoliasaurus. Whether the shorter or longer type of cervicals belong to it will remain uncertain until more remains are found. If it be a true Elasmosaurus, the figure will represent better a Cimoliasaurus.

In his second notice Leidy mentions his having reversed the extremities of the vertebral series in the three Cimoliasauri described by him.

5. *Lias and Oolite of Australia.*—Mr. CHARLES MOORE has published an important paper on Australian Mesozoic Geology, in the Quart. J. Geol. Society for 1870, describing beds of the age of the Middle and Upper Lias and Lower Oolite from Western Australia, and Oolitic or Lower Cretaceous from Queensland (at Wollumbilla). Out of 50 species of Mollusks in Western Australia 20 are found to be identical with British species, viz:

Ammonites Aalensis, var. *Moorei* Lycett, Upper Lias; *A. radians* Rein, Upper Lias; *A. Walcottii* Sow., Upper Lias; *A. macrocephalus* Schloth, Oolite; *A. Brocchii* Sow., Oolite; *Nautilus semistriatus* D'Orb., Upper Lias; *Belemnites canaliculatus* Mill, Oolite; *Gresslya donaciformis* Goldf., Upper Lias; *Maycites liassianus* Quenst., Middle Lias; *Cucullæa oblonga* Sow., Oolite; *Pholadomya ovulum* Ag., Oolite; *Avicula Münsteri* Goldf., Oolite; *A. echinata* Sow., Oolite; *Pecten cinctus* Sow., Oolite; *P. calvus* Münst., Oolite; *Lima proboscidea* Sow., Oolite; *L. punctata* Sow., Oolite; *Ostrea Marshii* Sow., Oolite; *Rhynchonella variabilis* Schloth, Oolite; *Cristellaria cultrata* Montfort, Oolite. *Lima proboscidea* and *Ostrea Marshii* appear to have been as abundant as in the hills around Bath; and *Pecten cinctus* from Australia attains the same large proportions as in this country. The Corn brash is apparently represented in Australia by *Ammonites macrocephalus* and *Avicula echinata*, and the Middle Lias by the *Mya cites liassianus* before mentioned.

The paper describes many new species.

6. *Plants of the Coal formation of Langeac, Haute-Loire;* by Dr. H. B. GEINITZ.—The plants mentioned in this paper as occurring at Langeac and remarked upon are *Calumites cannæformis* Schl., *C. Cisti* Bgt., *C. Suckowi* Bgt., *Annularia longifolia* Bgt., *Cyatheites arborescens* Sch., *C. dentatus* Bgt., *C. Miltoni* Artis, *Alethopteris pteroides* Bgt., *Curdiocarpus emarginatus* Göpp. & B., *C. Gutbieri*, *Noeggerathia palmæformis* Göpp., *Rhabdocarpus ovalis* Göpp. & F., *Cordaites principalis* Germ., *Trigonocarpus Nœggerathi* Sternb., *T. ventricosus* Göpp. & F. The several kinds of fruit here described give special interest to the memoir. The accompanying plate contains figures of several forms under six species, two species of Cardiocarpus, one of Rhabdocarpus, one of Cordaites, and two of Trigonocarpus.

7. *Sivatherium in Colorado.*—Dr. Leidy refers a fragment of a fossil from Colorado (received through Dr. Hayden from Dr. Gehrung), with a query to a genus near Sivatherium, which he calls *Megacerops.* He regards the fossil as corresponding to that portion of the face which comprises "the upper part of the nose, together with the forehead and anterior horn cores." The animal to which it belonged was nearly as large as the Sivatherium. The distance between the center of the two horn cores is $10\frac{1}{2}$ inches; the length of the horn cores above the intervening space 5 inches; the breadth of the face where narrow below the horn cores $7\frac{1}{4}$ in. He names the species *Megacerops Coloradensis.—Proc. Acad. Nat. Sci. Philad.*, 1870, p. 1.

8. *Megalonyx Jeffersoni in Illinois.*—A metacarpal bone of this Megalonyx is announced by Dr. Leidy (Proc. Acad. Nat. Sci., 1870, p. 13) as having been found in a crevice in the lead-bearing rocks near Galena, 130 feet below the surface, along with a last lower grinder of the extinct Ox, *Bison antiquus.* The Museum of the Academy contains other bones from the same locality, being remains of a large extinct Peccary, *Platygonus compressus*, an extinct Raccoon, *Procyon priscus*, and a large insectivore, *Anemodon Snyderi*, all of which were probably cotemporaries of the Megalonyx.

9. *Mineralogical Contributions of G. vom Rath* (Pogg. Ann. cxxxviii, 449).—Vom Rath describes and figures here crystals of the following Vesuvian minerals: twins of Anorthite, crystals of Oligoclase, Wollastonite, Orthite and Humite: besides giving an account of a new mineral from Laach which he names *Amblystegite*, and a description of certain twins of orthoclase.

Amblystegite occurs in small crystals of a brown to reddish-brown color, a gray streak slightly greenish; adamantine luster. The crystals are orthorhombic; $i\text{-}\bar{i}:\bar{1}=135°\ 50'$, $i\text{-}\bar{i}:1\text{-}\bar{2}=119°\ 26'$, $\frac{1}{4}\breve{i}:\frac{1}{4}\breve{i}=163°\ 47'$, $i\text{-}\breve{i}:\frac{1}{4}\breve{i}=98°\ 6\frac{1}{2}'$. B.B. very fusible and thus differs from humite and chrysolite. Composition,

$$\dot{S}i\ 49{\cdot}8\ \dot{\ddot{A}}l\ 5{\cdot}05\ \dot{F}e\ 25{\cdot}6\ \dot{M}g\ 17{\cdot}7\ \dot{C}a\ 0{\cdot}15=98{\cdot}30,$$

corresponding to the oxygen ratio for $\dot{R}, \ddot{R}, \dot{S}i$, 12·81 : 2·36 : 26·56. Only half a gram was here used. The mineral is so mixed with magnetic iron as to be separated with difficulty for analysis.

10. *Laxmannite* of Nordenskiöld (J. f. pr. Ch., cvii, 491) according to Hermann (ib. II, i, 447) is probably Vauquelinite; Nordenskiöld made the crystals monoclinic with the inclination of the vertical axis 69° 46′; G. = 5·77, H. = 3, color olive-green; and obtained for the composition (mean of two analyses)—

$$\dot{P}\ 8{\cdot}31\ \dot{C}r\ 15{\cdot}91\ \dot{P}b\ 61{\cdot}16\ \dot{C}u\ 11{\cdot}64\ \dot{F}e\ 1{\cdot}06\ \dot{H}\ 1{\cdot}10=99{\cdot}18$$

Hermann observes that Berzelius probably analysed the same mineral in his examination of the species he named Vauquelinite, and that if the pure oxyd of chrome of Berzelius be taken as a mixture of phosphoric and chromic acids, the two analyses closely agree, as follows:

	$\dot{P}$	$\ddot{C}r$	$\dot{P}b$	$\dot{C}u$	$\dot{F}e$	$\dot{H}$	
Berzelius,	8·31	15·85	60·87	10·80	1·10	1·10	=98·03
Nordenskiöld,	8·31	15·91	61·16	11·64	1·06	1·10	=99·18

both leading to the formula $2\dot{C}u^3\dot{P}+5\dot{P}b^3\ddot{C}r+2\dot{H}$.

11. *Phosphorchromite.*—Hermann (J. f. pr. Ch., II., i, 450) thus names a chromophosphate of lead and copper from Beresofsk. It occurs concretionary, crystalline or massive within, with a blackish-green color, pistachio-green streak, H. =3, G. =5·80. An analysis afforded:

$\dot{P}$ 9·94 $\ddot{C}r$ 10·13 $\dot{P}b$ 68·33 $\dot{C}u$ 7·36 $\dot{F}e$ 2·80 $\dot{H}$ 1·16=99·72,

leading to the formula $3\dot{C}u^3\dot{P}+5\dot{P}b^3\ddot{C}r+3\dot{H}$.

Vanadiolite of Hermann (J. f. pr. Chem., ib. 445) occurs in small crystals, partly in druses, of a dark or blackish-green color and gray-green streak; dark emerald-green color and transparent, in small grains; of strong vitreous luster, with G. = 3·36. B.B. melts on the edges to a black blebby slag. His analysis afforded

$\dot{S}i$ 15·61 $\ddot{V}$ 44·85 $\ddot{A}l$ 1·10 $\dot{F}e$ 1·40 $\dot{M}g$ 2·61 $\dot{C}a$ 34·43=100

and he regards it as a subvanadate combined with augite like the Lavroffite of the same locality, Slüdänka, near Lake Baikal. He writes the formula $3\ddot{R}\dot{S}i+\dot{C}a^6(\dot{V}O^4+2\dot{V}O^5)$.

12. *Wolframite.*—Descloizeaux suggested in 1850 (Ann. Ch. Phys., III, xxviii) that Wolfram was oblique rhombic in crystallization, after some measurements of crystals. He has recently confirmed this conclusion (ib., IV, xix, 168, Feb. 1870) by both optical and crystallographic observations. He finds the obliquity 90° 38′, and the prismatic angle 100° 37′. His crystals were from Bayewka, near Ekatherinenbourg, in the Urals, a variety which afforded Mr. Koulibine,

$\ddot{W}$ 74·32 $\dot{F}e$ 2·11 $\dot{M}n$ 20·90 $\dot{C}a$ 1·30 $\dot{S}i$ 0·28=98·91,

agreeing nearly with Hubnerite, of Nevada. G.=7·357.

13. *Namaqualite, a new ore of Copper;* by Prof. Church (J. Ch. Soc., II, viii, 1).—Namaqualite is named after the region in which it occurs, Namaqualand, S. Africa. It occurs in thin layers of silky fibers, which alternate with chrysocolla and are sparingly mixed with small crystals of "magnesia mica." Color pale blue; H.=2·5; G.=2·49; isolated fibers transparent under the microscope. In the closed tube yields much water and blackens; at 100° C., or over sulphuric acid in vacuo, no loss of weight. Mean of analyses:

$\dot{S}i$	$\ddot{A}l$	$\dot{C}u$	$\dot{M}g$	$\dot{C}a$	$\dot{H}$
2·25	15·29	44·74	3·42	2·01	32·38=100·09.

The oxygen ratio for the protoxyds, alumina and water is 4 : 3 : 11. Prof. Church remarks that its crystallized condition, and its definite and constant composition show that it is a true species; and the presence of a sesquioxyd that it is related in composition to hydrotalcite and pyroaurite.

14. *Contributions to the Mineralogy of Victoria;* by G. H. S. Ulrich, F.G.S. 32 pp. 8vo. Melbourne, 1870. (Reprinted from

the Report on the Mineral Statistics of Victoria for the year 1869, presented to Parliament.)—These "contributions" contain detailed descriptions of many species, with figures of crystals, and some analyses, the latter by Mr. C. Newbery. The following are among the new or more interesting species noted by Mr. Ulrich.

Maldonite, or Bismuthic Gold, from the Nuggety reef, of the Alliance Company, Maldon. It occurs in particles in the granite veins affording gold. It has a pinkish-white color, but tarnishes easily on exposure to a dull copper color, and ultimately to black. Cleavage apparently cubic. H.=1·5—2. Malleable and very sectile. Formerly in larger pieces in the upper workings of the Alliance Company's mine.

Native Bismuth, Bismuth Glance and Bismuthite occur in a quartz vein at Linton, Ballarat district.

Selwynite of Ulrich (Dana's Min., p. 509*), a chrome-bearing mineral, has been further examined. Mr. Newbery obtained in new analyses—

Si	Al	Cr	Mg	Na	H
47·25	35·28	7·82	2·42	—	5·67
48·42	34·72	6·94	2·11	2·03	4·83
48·23	38·16	6·14	1·21	3·12	2·90

the mean of which affords very nearly the oxygen ratio for R, R, Si, H, 1 : 16 : 22 : 3½. Mr. Ulrich observes that the mineral is probably an altered feldspathic mineral, and related to the Pinite group, or especially to gieseckite and dysyntribite. When polished it has some resemblance to nephrite.

Talcosite is a new mineral occurring in thin seams in the selwynite—the seams lamellar-columnar. Resembles talc in feel. H.=1; but transverse to lamination 1·5–2·0. G.=2·46–2·5. Color silver-white, faint greenish or yellowish. Luster pearly. Scales flexible, not elastic. B. B. exfoliates, whitens, gives off water, and fuses at 4 to a blebby enamel; with cobalt solution a fine blue. A mean of two analyses by Mr. C. Newbery—

Si 49·04 Al 45·98 Cr, Fe, Mg, Na, *tr.* H 4·30=99·32,

giving the oxygen ratio of R, Si, H=5 : 6 : 1.

Herschelite is found at a quarry of basalt near the river Yarra at Richmond. Several interesting figures of its crystals are given. It occurs with phillipsite, analcite and calcite. It is also found in basalt at the shaft of the Ballarat and Clunes Gold-Mining Co., Clunes.

Struvite in crystals occurs in guano, in the Skipton Caves near Ballarat. The depth of the guano is about 20 feet, and it has been derived from the *excrements of bats.*

Tetrahedrite, Stibnite, Molybdenite, Brookite, Cassiterite, Scheelite, Sapphire, Topaz are among the other minerals of which mention is made in the memoir.

15. *Lavroffite* (Lawrowit), has been analyzed by Hermann (*J.* f. pr. Ch., 1870, II, i, 444) and shown to be a vanadiferous diopside.

* In the analysis cited in the Mineralogy, Mg 4·36, should be Mg 4·56.

He obtained $\dot{S}i$ 53·65 $\dot{A}l$ 2·25 $\dot{F}e$ 2·48 $\dot{M}g$ 16·00 $\dot{C}a$ 23·05 = 100, with 2·57 p. c. of hypovanadic acid.

16. *On Sellaite, a new native fluorid*, by Dr. STRUVER, Atti della R. Accad. di Torino, iv, 1868, 35.—This mineral was detected on a specimen of anhydrite from Geibroulà, in the State of Maggiore, in Piedmont. On the anhydrite there were also crystalline sulphur, dolomite, and rare twins of albite. It is tetragonal in crystallization, with I on 1 (∞ P on P)=123° 30′, and *i-i* on *i*-3 (or ∞P∞ on ∞P3) =161° 34′. Cleavage parallel to I and *i-i* perfect. H.=5. G.=2·972 at 24° C. Fracture conchoidal. Luster vitreous. Colorless. Transparent. Powder white.

It is insoluble in water; also in acids, except concentrated sulphuric, which causes an evolution of fluohydric acid. Small fragments melt in the flame of a candle with intumescence. From the similarity of its reactions to those of fluorite, the author concludes as not improbable that the mineral is a monofluorid of magnesium, which contains 38·71 of magnesia and 61·29 of fluorine. In treating a small fragment of it with concentrated sulphuric acid, he obtained for the proportion of magnesium 39·64 p. c. The small amount of the mineral in hand prevented his making a complete analysis. The species is named after the distinguished crystallographer, QUINTINO SELLA.

17. *Ambrosite.*—C. U. SHEPARD describes under this name, in The Rural Carolinian, Feb., 1870, p. 311, a resin resembling amber, from the phosphatic formation near Charleston, S. C. It is yellowish-brown externally, and clove-brown within; feebly translucent; sinks slowly in water; and melts into a clear liquid at 460° F., after softening at a much lower temperature. It gives off much succinic acid long before it melts, and a dense yellow oil is volatilized on its fusion. It is very combustible, burns with a bright yellowish-white light and pleasant odor, and leaves no ash behind. The name is made from the two words *amber* and *rosin*.

18. *On the Guanape Island Guano and its minerals;* by Prof. C. U. SHEPARD, (ib. May, p. 469).—Guanape island is two miles northeast of the Chincha Islands. A mineral occurring in balls and veins in the Guanape Guano, having H. =1·5, G. =2·3, and resembling a little the red Cheshire rock-salt, but rhombic instead of cubic in cleavage, is called *Guanapite* by the author. He found it to consist of sulphate of potash 67·75, sulphate of ammonia 27·88, oxalate of ammonia 3·75=99·38. It loses ammonia slightly on exposure to the air.

Another substance from the Guano is named *Guanoxalite*. It contains sulphate of potash 40·20, oxalate of ammonia 29·57, water 30·46 =100·23. It occurs as pseudomorphs of a bird's egg, having the size and shape of that of a domestic duck. Color white exteriorly, but from the presence of the altered shell. Within, the material is foliated with a rhombic cleavage; a cream-white color; somewhat pearly luster; feeble translucence; hardness below 2, and G. =1·58. When heated it swells up, blackens, partially fuses, gives off copious fumes of ammonia, and leaves a white residue of sulphate of potash.

In the cavities of the spinal column and the stomach of the remains of birds from the Guano, yellow crystals of the mineral *Taylorite* occur, a species first noticed by W. J. Taylor in the guano of the Chincha Islands. Along with the Taylorite there are numerous minute scaly crystals of *Aphthitalite;* also oxalate of ammonia, which Prof. Shepard names *Oxammite. Oxalate of lime* is occasionally observed in crystals; also phosphate of ammonia (*phosphammite* of Shepard) in crystals and lumps; and, as an efflorescence from this mineral, biphosphate of ammonia (*Biphosphammite* S.).

III. BOTANY AND ZOOLOGY.

1. *Miscellaneous Botanical Notices and Observations.*

The Code of Botanical Nomenclature, as digested by M. Alph. DeCandolle and adopted by the International Congress at Paris in 1867, while on the whole approved by botanists, has been variously criticised in certain particulars, some writers naturally objecting to one article, some to another. To the more important of these criticisms DeCandolle made reply at a meeting of the Botanical Society of France on the 26th of February, 1869. The number of the *Bulletin* which contains his communication was issued, we believe, at the beginning of the present year. M. DeCandolle intimates that he passes by the still controverted question as to the proper mode of citing the authority for species where they have been transferred to another than the original genus, as being one upon which further discussion could hardly be more than iteration. He merely remarks that the more experience we have of the working of the system of double citation of authors, whether with or without parenthesis, the more grave do the inconveniences appear. Since we cannot readily make a brief abstract of the several points which he does discuss, we will now notice only those upon which this Journal ventured to differ from M. DeCandolle and the Congress. The chief serious objection was raised upon article 50, which prescribed the mode in which unpublished names, taken up by an author, should be subsequently cited. We remarked that: "For instance, there may be no necessity for taking up one of Commerson's names affixed by him to his plants in herbaria; but if taken up, simple verity would seem to require this botanist's name to be cited. We should feel bound to write '*Flacourtia* Commerson,' although published by L'Heritier or Jussieu, who probably supplied the character. The rule as proposed would apply to names communicated with manuscript characters by one botanist to another, as well as to named specimens. Now, no botanist is bound to do the work of publication for another; but if he chooses to do so, the maxim *qui facit per alias*, etc., must [conversely] fairly apply, and succeeding writers should not be required to take the godfather for the father. If we rightly understand the editor, he proposes that we should write *Eulophus*, *Leptocaulis*, and *Trepocarpus* DC., although the elder DeCandolle,

accepting these names with the specimens from Nuttall, scrupulously attributes them to 'Nutt. in litt.' To us, all such names, which the elder DeCandolle has, at his own discretion, published for Nuttall, are of *Nutt. in DC. Prodr. &c.*"

To which M. DeCandolle now rejoins: . . . . "But see the inconvenience which results. All the catalogues or dictionaries, and all indexes have *Leptocaulis* Nutt. So we search the works of Nuttall, and perhaps even the small papers scattered in the journals, and lose much time and pains; for Nuttall never published this name. It may be that he would not have desired to publish it if he had examined the question subsequently. The date of the genus is that of the publication; and the publication is really the one essential thing; for what are the most important discoveries if not published? In writing *Leptocaulis DC. ex Nutt. litt.* the primitive author is equally indicated; but then one will see in the books *Leptocaulis DC.*, and any one will readily find in the works of DeCandolle the origin of the genus and the date of publication."

We are disposed to add that the information sought would be as readily found when written "*Leptocaulis Nutt. in DC.*," and that this more strictly as well as more tersely represents the fact than "*Leptocaulis DC. ex Nutt. litt.*" The full reference is "*Leptocaulis* Nutt. in DC. Prodr. 4, p. 107;" and the question is, whether in mere enumeration this is to be abbreviated into "*Leptocaulis Nutt*," or "*Leptocaulis DC.*" Either way, the work, as well as volume and page, has to be looked up, and the trouble of finding the origin of the name in the pages of the Prodromus may probably be far less than if Nuttall had published it directly in one of his various scattered papers. But let us in our turn test the new rule by the results which would ensue from its consistent application. To the first volume of the Flora of North America Nuttall contributed a large number of new species and genera, which were published for him by the authors of that work; but for which, if cited according to the new rule, they would seem to have incurred a responsibility much beyond what was counted on. They might well insist that the new mode of citation was misleading. Moreover, if "*Centrostegia* A. Gray, mss.," so published in Bentham's *Eriogoneæ* contributed to DeCandolle's Prodromus, is briefly to be cited "*Centrostegia Thurberi* Benth. in DC. Prodr," must not *Eriogonum Douglasii* and a score of other such species on the same principle be cited as *E. Douglasii* Alph. DC., &c.? for we should search the works of Bentham in vain for these names, losing much time and pains. It will of course be said that the "auctor Bentham" at the top of the page explicitly indicates the actual author; but so equally does the "A. Gray mss." at the head of that article. To take another instance: upon this plan we may be required to write "*Eliottia* Elliot" (Sketch, 1, p. 448). Elliott himself wrote "*Elliottia* Muhlenberg," adding, "I have inserted it, as requested by Dr. Muhlenberg, under this name." He adds to the specific name "Muhl. Cat." indeed. But the context shows that the second edition of Muhlenberg's Catalogue was not

then published; it is doubtful if it was so when that part of Elliott's first volume was issued; and if it were, the enumeration in the catalogue, according to the Code, is not tantamount to publication. There are other well-known cases of a similar sort, in which an author would become liable to the imputation of dedicating a genus to himself.

When we consider how punctilious the elder DeCandolle was in this respect and others of a similar sort, and how generally he has been followed, we must regard the change proposed in the code and exemplified in some (but not all) of the later volumes of the Prodromus as an innovation rather than the declaration of an existent common law, or a natural development of it. The laudable end sought, however, in this and in article 42, is to obviate or diminish grave and growing inconveniences which arise from imperfect publication and merely recorded names in collections and herbaria. And here we do not complain that the rights of publication through the distribution of specimens, conceded in art. 42, are seemingly very much restricted by the new commentary. The duty, or perhaps comity, of respecting imperfectly published or unpublished names is only one of imperfect obligation, as the moralists would say, and therefore not to be fixed by law but governed by discretion. Legal rights begin only with publication, no matter by whose agency. We would only urge that subsequent citation, purporting to indicate the origin, should not in effect misquote the record.

Baillon's Histoire des Plantes, briefly noticed in our January number, goes on well. We have received two more monographs published since the year began; one of the *Proteaceæ*, the other and much larger one of the Papilionaceous *Leguminosæ*. The paper and print are most excellent and attractive: so also are the wood-cuts, as is usual in French publications of this kind. We notice that an English translation is announced. Considering the high price and generally limited sale of botanical books in England we should not expect it would pay. But, if it could ever be finished upon the plan adopted, it would be a proper companion and complement to LeMaout and Decaisne's *Traité General de Botanique*, of which an English edition is now in preparation under the best auspices. The latter copiously illustrates the families, and barely enumerates some of the principal genera. The former illustrates the tribes, &c., by a general history of the structure of some leading representatives, cast in a popular and readable form, and adds the characters of the genera, in the ordinary technical form and in the Latin language.

An attempted Improvement in the Arrangement of Ferns and in the Nomenclature of their Subdivisions, is the title of a pamphlet by the Rev. Prof. Hincks of the University of Toronto and President of the Canadian Institute; a popular account of the structure, fructification, and fertilization of this group of plants, which he regards as an alliance (*Filicales*), containing the three orders, *Osmundaceæ*, *Cyatheaceæ*, and *Polypodiaceæ*, founded on

the sporangia, the *Ophioglorsaceæ* being excluded by their "straight æstivation" [vernation] and referred, as is natural, to the *Lycopodiaceæ*. The tribes of the first two filical orders three each, of the *Polypodiaceæ* four times three, or three for each division or subdivision, founded on the sori. For genera he is disposed to make more of the venation than is the present mode, and less of stipes and articulation, and so discards *Phegopteris*. Finally, he takes up the questions relating to *Aspidium*, both as to generic comprehension and nomenclature, and proposes to keep *Lastrea* and *Polystichum* as genera.

The Genus Hydrolea is reviewed by A. W. Bennett in the 53d number of the Journal of the Linnean Society (May, 1870), and the species are brought up to thirteen, of which three are here established. That *H. affinis*, described in the last edition of Gray's Manual, should have been overlooked is not to be wondered at; but specimens of it doubtless occur in the Kew herbaria, probably confounded with *H. quadrivalvis*. Mr. Bennett sides with Choisy in keeping up the order *Hydroleaceæ*, as distinct from *Hydrophyllaceæ*, "most clearly," on account of "the biocular many-seeded ovary, axile placentation, and leaves invariably simple." These characters come down to two, the indefinitely numerous ovules and the two-celled ovary. But the new Californian genus *Draperia* Torr. stands in the gap, having the two-celled ovary as well as the habit of *Nama*, with the geminate ovules of proper *Phacelia*.

Tampico Jalap has within the last few years come into the markets as a distinct sort, and "although less rich in resin and less purgative than true jalap, yet on account of its lower price it has found a ready sale." Mr. Daniel Hanbury has traced the article to its source, in the root of a new *Ipomœa*, *I. simulans* Hanbury, described and figured in the 53d number of the Journal of the Linnean Society, which inhabits the Sierra Gorda, etc., in Mexico.

A Revision of the Flora of Iceland is the title of an extensive paper in the same Journal, by Professor Babington. The catalogue of Phænogamous and Acrogenous plants reaches 467 species. Although Iceland touches the arctic circle and is largely occupied by mountains, "many of which rise to the height of 6000 feet and are covered through fully their upper half with perpetual ice and snow, from whence extensive glaciers descend almost to the level of the sea," yet the climate is rendered so mild comparatively by the influence of the Gulf Stream which washes the coast, and the rain-fall and prevailing cloudiness in summer are so great, that, while on the one hand there is no proper forest, on the other "only 62 species are found which do not form part of the British Flora," and not more than three are decidedly arctic, viz., *Gentiana detonsa*, *Pleurogyne rotata*, and *Epilobium latifolium*." All three of these come down to comparatively low latitudes and low levels in North America. A plant of much interest, and which should be thoroughly compared with that so named in North

America, is *Platanthera hyperborea*, of which the present writer craves Icelandic specimens.

The Fertilization of various Flowers by Insects is the title of an original paper, contributed by Dr. Wm. Ogle to the April number of the Popular Science Review. Of the various mechanical contrivances described, as also of the views suggested, some are not novel, the writer, as he states, not being familiar with the botanical literature of the subject (and this is most evident): but the whole article is well worthy of much attention, and would have been most interesting to the general reader—the narrative being remarkably clear—except that it is so disfigured by typographical errors as to be almost unintelligible to ordinary readers; indeed, the proofs would seem not to have been revised at all. The most interesting and original portion is that which relates to the fertilization in the Heath family, and to the use of the awns or appendages on the back of the anthers in Heaths, Uva-Ursi, Vaccinium, &c., by which the bee in sucking the flower topples or moves the anther, so that the pollen falls out of the terminal openings upon the insect's head. Bean-flowers and their fertilization, as also in related papilionaceous blossoms, are well described; and it is noticed that, while some bees have learned to get at the nectar feloniously by making a hole in the side of the calyx-tube, and others enter the regular way, and so do their proper work, an individual bee, visiting a succession of bean-flowers, keeps persistently to the one or the other plan. "It would thus appear that the habit is not an instinct, belonging by inheritance to the whole species, but is in each case the result of individual experience. As with the same experience some bees have acquired the habit and others have not, we must admit not only that these insects are intelligent, but that they differ from each other in their degrees of intelligence, some being slow in acquiring knowledge, others quicker." Perhaps the knowing ones have inherited the knack, in which case it is instinct in them, after all. Instinct, briefly defined, is congenital habit.

Eléments de Botanique et de Physiologie Végétale, suivis d'une petite Flore simple et facile pour aider à découvrir les noms des Plantes les plus communs du Canada: par l'Abbé Ovide Brunet, Prof. Bot. à l'Université Laval. Quebec, 1870.—A very neat little book, prepared by Prof. Brunet, as a sort of first book in Botany for Lower Canada, where French is still the language of instruction. It is illustrated by wood-cuts mostly from very good drawings by the author's own hands; and the structural and physiological part, and also the little flora, seem to be as clear as they well can be in the diminutive space they occupy; excepting that *Abies Canadensis* is put with Balsam-Fir into a genus in which the scales are said to fall from the axis, which is an obvious oversight. It is curious to see what the plants are which are taken as the commonest in Lower Canada, and what the popular names are. For instance, *Sarracenia* is known as "Petit-cochons;" Wood-sorrel as "Alléluia;" *Impatiens fulva* as "Chou-sauvage;" *Hama-*

melis as "Café-du-Diable;" Cranberry as "Atoca," but *Vacinium Vitis-Idœa* as "Pommes-de-terre."

The Herbarium of the late Von Martius has been purchased for 30,000 francs by the Belgian Government, to form the nucleus of a national collection at Brussels.

The Michaux Grove Oaks.—This name is to be applied to a collection of Oaks to be planted in Fairmount Park, Philadelphia, "in which, if practicable, shall grow two oaks of every kind that will endure the climate," in commemoration of the younger (F. A.) Michaux, author of the Sylva Americana, and his father, author of *Histoire des Chênes de l'Amerique.* The grove is to be maintained by the income of a legacy of $6000 bequeathed by the younger Michaux to the American Philosophical Society, in trust for arboricultural purposes, which legacy is now devoted by the Society to this use.

Otto Bœckeler is publishing in the current volume of the *Linnæa*, a full account of the Cyperaceæ of the Royal Hebarium of Berlin, which now, in the fourth fasciculus reaching the *Scirpeæ*, is becoming of consequence to United States botanists. He describes several supposed new species of *Cyperus* and a few of *Eleocharis*, &c., and identifies several of ours with older species, making some changes of nomenclature. By what may be a good hit he refers the inner squamula of *Hemicarpha* to an abortive stamen, and so refers the species to Scirpus, ours becoming *S. micranthus* Vahl.

Generis Astragali Species Gentogœæ pars altera.—The first part of this monograph by Prof. Bunge of the Old-World species (almost a thousand in number) of the great genus *Astragalus*, was mentioned in our January number. We have now received the completion, containing the systematic enumeration of the species, with characters and descriptions of most of them, and the synonymy. The work is published by the Imperial Academy of St. Petersburgh, this part being the initial number of the 15th volume of its *Mémoires*, pp. 254.—The succeeding number, of over 300 pages, consists of part first of

Flora Caucasi, by Rupprecht, with six plates. It carries the work from *Ranunculaceæ* down to *Vitaceæ*. The species are described or annotated at large, but not by definite technical characters, and vast observation and learning are exhibited, along with a liking for multiplication of genera. Dr. Rupprecht considers the name *Viola umbrosa* of Fries (1828) to have precedence of *Viola Selkirkii* Pursh ex Goldie, 1822, on the ground that the latter name was not published until 1833 by Hooker. But in Goldie's paper (which indeed was drawn up by Hooker) there is a good Latin diagnosis of the species followed by almost half a page of detailed description in English; in fact, hardly any species has been more effectually published.

Mr. Bentham's Presidential Address at the anniversary meeting this year is somewhat briefer than usual, but not less likely to attract attention. It is mainly devoted to two topics: 1, the results obtained from the recent explorations of the deep-sea faunas; and

2, those from the investigation of the tertiary deposits of the arctic regions, "tending both of them to elucidate in a remarkable degree one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods." After briefly indicating where the principal data are recorded, Mr. Bentham continues:

"It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previously conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side, without requiring for the latter any general catastrophe that should preclude the former.

"There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea by the gradual growth and decay of Globigerinæ and the animals that fed on them —amongst others, for instance, *Rhizocrinus*, and *Terebratulina caput-serpentis;* and when, at a later period, the upheaval of the ground into an element where these animals could no longer live arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed; and so, on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological periods they have witnessed. So also there was a time when the hill-sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were, under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and many others); and when at a later period these forests were destroyed by the general refrigeration, the Taxodium already occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some spot where it has gone on and thriven, and continued its race from generation to generation down to the present day, unchanged in character, and unmodified in its requirements. In

both cases, the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas; others, again, had spread like the permanent ones, but were less fitted for the new conditions in which they had become placed, and in the course of successive generations have been gradually modified by the Darwinian process of natural selection, the survival of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Dr. Duncan, that 'this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods;' but we can, at the same time, feel that Dr. Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed, from its first commencement to the present day, in unbroken continuity and unchanged in character."

A portion of this address will probably astonish the vegetable paleontologists, excepting perhaps, Mr. Lesquereux, whose cautious language, and his statement in this Journal that, properly speaking, no *species* can be established from leaves or mere fragments of leaves, are commended. For the President of the Linnæan Society avows himself wholly skeptical as to the "New Holland in Europe" of eocene times, denying the existence of a single specimen out of the nearly one hundred supposed tertiary species which a modern systematic botanist would admit to be Proteaceous, unless received from a country where *Proteaceæ* were otherwise known to exist. The grounds of this opinion are given in detail.

The Student's Flora of the British Islands, by J. D. Hooker, C.B., &c., just issued by Macmillan & Co., comes to take the place of Hooker & Arnott's British Flora, now out of print and antiquated, as that took the place of Sir William's British Flora which did such good service in its day and in its five editions. In these a goodly octavo, then a thick duodecimo, it now becomes a compact 16mo, a very "handy book," which may be carried in the

pocket; but this compactness is not at the expense of either clearness or fullness. "The object of this work," so the preface opens, "is to supply students and field-botanists with a fuller account of the plants of the British Islands than the manuals hitherto in use aim at giving." The body of the volume, exclusive of Synopsis and Index, is contained in 474 pages; the species appear nearly to average three to the page. We are glad to see that orthodox specific characters are kept up, and that they are specific characters, diagnoses, not descriptions. Descriptive matter follows and, with habitat, range in elevation, and distribution in general, forms a secondary paragraph of much greater extent than the diagnosis; and then "*sub-species*" are numerous and fully characterized, as are occasionally varieties of second order under them. The species are naturally arranged, under sections when needful; but keys to the species are not given, the author "finding from experience that such keys promote very superficial habits amongst students,"—which they certainly do. As the book is wholly new, and the descriptions all at first hand, from living plants and dried specimens, a good many slips and oversights are inevitable. With the longest experience and great painstaking these cannot be avoided, but have to be weeded out at leisure. Here where a British Flora is only incidentally used, we have no call to point out such as we have noticed, most of which are sure to be rectified in the new edition, which must needs be promptly called for. But we will venture to find fault with one typographical blemish, as we think it, viz., the insertion of the accent mark between the letters in generic and specific names, so as to display an unsightly cleft in the middle. Surely the London printing-houses have, or can have, the vowels with accents cast upon the type; and there are ways of indicating whether the vowel be long or short, if that be worth the while. This whole business of accentuation is, no doubt, of secondary consequence, and plainly is not so much considered by English botanists of the present day as by those of a past generation. But ordinary students and amateurs may fairly ask for guidance; and those who have occasion to coin many botanical names are bound to consider in advance how they must needs be pronounced. Artificial keys to the natural orders are liable in some degree to the same objection as are keys to species; but they are almost indispensable to young students; only they must be made with extreme care, and every deviation or exception provided for. A synopsis of the natural orders, such as Dr. Hooker has prepared, guarded by mention of exceptions and qualifications, has its own advantages, and where the number of orders is not larger than in Britain may perfectly serve the purpose. A. G.

The American Entomologist, now published at St. Louis, has this year added the words "and Botanist" to its title, and Dr. George Vasey has taken charge of the Botanical Department. He has entered upon his duties with promising vigor, and is producing a series of popular botanical articles which are likely to be widely read throughout the western country and to be very useful.

In the combined July and August number the editor has a spirited article upon the Origin of Prairie Vegetation, which is mainly a criticism of the writings of Prof. Winchell upon this subject in his "Sketches of Creation." If it is seriously maintained in that work that seeds buried in diluvial deposits may have retained their vitality during the glacial period, and by their germination after it "reproduced the flora of the præ-glacial period," we can only wonder that this immense extension of the hypothesis of the indefinite vitality of buried seeds should have been made at a time when most biologists probably doubt whether any seed ever preserved the power of germination for a century or two. A. G.

2. *Carbolizing Birds;* by H. W. PARKER. (Communicated by the Author.)—The following methods, carefully studied for two years, with results noted, are recommended for the saving of birds in warm weather until the operator finds time to skin them; for the permanent preparation of drawer specimens, where the student needs a large series of individuals to determine the variations and limits of species; and for mounting small birds, at least as temporary representatives, when neither the time nor the expense involved in the old methods can be afforded.

The viscera are removed, to effect which neatly the legs are pinned widely apart, and a paper several times folded is pinned over the tail in the direction whither the viscera are drawn out. With proper care, the sex is readily observed. A wad of cotton absorbs the fluids remaining in the cavity. The leg is then grasped close to the body, and a knife or wire is introduced into the cavity and run down into the flesh of the leg, working the instrument around, but not so as to break the skin. For a small bird, five to ten drops of the commercial fluid preparation of carbolic acid is made to anoint the whole interior, and to penetrate the leg by stretching and relaxing the same in proper position. The application is repeated after the first drops are absorbed; and a wad of cotton, wet with the acid, may be left close under the breast-bone next to the neck. The cavity is then filled with cotton and the skin drawn back into place. The inside of the mouth is well anointed, and a saturated wad of cotton pushed down the whole length of the neck. The eyes are removed by a hooked wire inserted into the ball, the head being so held that the humors of the eye will drop without soiling the lids. The moist lids are left as open as possible, and the specimen placed in a cool cellar till the next day, when the lids are dry enough to take their open shape. Then a nail is inserted through the lids and pushed through the bone at the back part of the orbit into the brain, and so worked as to make a good opening. A tightly rolled bit of cotton, saturated with the acid, is pushed into the brain and worked around in it, care being taken not to wet the eyelids. If by chance the feathers are wet, the acid can be removed by powdered chalk, repeatedly applied.

Specimens so prepared in warm weather, can be skinned a week or two after, if kept boxed in a cellar. No smell of decomposi-

tion is observed; the acid gradually and completely penetrates the pectoral muscles; the skin is strong and the feathers not loosened.

For permanent preparation, the skin should be laid open from the abdomen to the neck, the pectoral muscles removed and replaced by cotton, and the incision sewed up. The throat, neck and orbits are also filled with cotton. The specimen should then be suitably arranged, encircled by a slip of paper, and placed on a bed of cotton. Before this, the flesh of the wings should be laid open and arsenic applied in the usual manner.

For mounting, it only needs to run one wire through the foot, tarsus, and so on through the neck to the forehead, and another wire through the other foot to any point in the back or breast where the end of the wire catches firmly. Papers or strings for keeping the feathers in place should remain long. Some shrinking about the head and neck will eventually follow in the case of many birds, particularly those of the smallest size or of scanty, or close, plumage; but in other instances no shrinking whatever can be noticed after more than a year of drying. The cabinet in which they have been set up is made insect-proof by means of pasted cloth and paper, putty and paint, fifteen inches passage way being left in front of the shelves and the only access being through a tight door at one end, fastened by a screw.

Travelers, who desire to collect a large number of birds for comparison, will find this method one of great advantage; and the specimens will be better for study than skins, inasmuch as the proportions will be better preserved. Small mammals can be kept some days for skinning by a similar process, and an opening into the brain may be made through the roof of the mouth, if preferred. A Fox Squirrel, so treated, was in good condition for skinning after four day's preservation, in very warm weather.

Iowa College, July, 1870.

This with all similar methods of preparing permanent specimens, without skinning, has been found to be of comparatively little use in the damper air of the Eastern States, especially near the coast, where all dried preparations are so liable to mould and decay.—Eds.

3. *A Synopsis of the Family Unionidæ;* by Isaac Lea, LL.D., Vice-Pres. Amer. Phil. Soc., &c. 4th ed., very greatly enlarged and improved. 184 pp. 4to. Philadelphia, 1870. (Henry C. Lea).—Dr. Lea has given further completeness to his labors on the Unionidæ by preparing and publishing this fourth edition of his Synopsis. During the eighteen years that have elapsed since the issue of the preceding edition, the number of known species has much increased, and various corrections of former determinations have been made. The subject of the arrangement of the species in genera is discussed in the earlier part of the volume, and then tables are given, with very full synonymy and numerous annotations. The table of geographical distribution, which next follows, is very much enlarged. The volume closes with an Index of all the names of species and a statement of the place of publication of each, and finally a long Bibliography.

4. *Commensalism among Animals.*—Van Beneden, at the meeting of the Royal Academy of Belgium, on the fifth of March last, continued his observations on commensalism, or the living together of different species of animals. He first cited the following facts which he had received from Mr. Al. Agassiz. A species of *Lepidonotus*, of California, is always found attached on an *Asteracanthion* (*A. ochraceus* Brandt) near its mouth, on different parts of the ambulacral rays, sometimes five of them on a single individual. A small fish of the genus *Clupea* is often found lodged in the folds of the fringes of a species of Pelagia (*Dactylometra quinquecirra* Al. Ag.) A species of *Hirudinea* lives within the cavity of a Beroë (*Mnemiopsis Leydii*) of Buzzard's Bay, the Beroë never being seen without four or five of these worms, and often harboring seven or eight. Between the buccal fringes of the large Medusa *Cyanea arctica* (as stated in the Seaside Studies), a species sometimes 7½ feet in diameter, there lives an Actinia (*Biccidium* of L. Ag.); three to five of these Actiniæ reside commonly on each Cyanea. In an *Aurelia* of our coasts, a large number of Crustacea of the family Hyperina are often harbored. Another interesting fact is the commensalism of the young Comatulæ on the adult. The young of the species of the coast of South Carolina attaches itself to the basal cirri, and they live like a small colony of young Pentacrini. A species of Planaria, the *P. angulata* Müller, lives always in free "commensal" on the under surface of the American Limulus, near the base of the tail.

Van Beneden next speaks of the relation of the siliceous *Hyalonema* to the associated polyps (a species of *Polythoa*), and makes it as a case of commensalism of polyps on a sponge. He adds the fact that Mr. Oscar Schmidt has found in the Adriatic a *Polythoa* living on a species of sponge of the genus *Axinella*. He closes with repeating an opinion he had before expressed (in the *Zool. Médicale*, published long since by him in connection with *P. Gervais*) that sponges are only polyps of extreme simplicity, in which the active part is reduced to a membranous tube without tentacles about the orifice.—*L'Institut*, July 20.

IV. ASTRONOMY.

1. *Discovery of a new Asteroid, the 111th;* by Dr. C. H. F. Peters. From a communication by the author to one of the editors, dated, Litchfield Observatory of Hamilton College, Clinton, N. Y., Aug. 16, 1870.—I take pleasure in forwarding the first observation made upon an asteroid, the 111th of the group, discovered here night before last, viz:

1870.	Aug.	14.	h 12	m 38	s 31	H. C.	m. t.	A.R. =	h 21	m 25	s 20·21	Decl. =	° −13	′ 10	″ 5·4	10 Comp.
	"	15.	9	5	40	"	"		21	24	32·88		−13	12	29·6	5 "

The planet was of about 11½ magnitude.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Nineteenth Meeting of the American Association for the Advancement of Science, held in Troy, New York, August* 17–25, 1870.—The meeting of the American Association which has just closed at Troy, was, in point of interest and value, one of the best in its history. Owing to the continued illness of President William Chauvenet of St. Louis, the duties of President were performed by the Vice President, Dr. T. Sterry Hunt, of Montreal. And in the absence of Professor C. F. Hartt of Ithaca, the General Secretary, now in Brazil, Professor F. W. Putnam of Salem was elected to that office for the session. The attendance at the meeting was good, the names enrolled upon the Treasurer's book at the close of the session being about two hundred. The titles of papers entered with the Permanent Secretary numbered one hundred and forty-three. And, though there was no one subject of great general interest under discussion, as at Chicago and Salem, yet the absolute value of the papers read at Troy was quite equal to that of those presented at either of the meetings mentioned.

Of the papers read in General session, that by Lieut. C. E. Dutton, U. S. A., upon the Chemistry of the Bessemer Process, deserves particular mention. It was an admirable analysis of the process itself, and also of the successive stages which are noticed in the conversion. An abstract of the paper, by Professor Barker, is deferred to our next number.

The address of Col. J. W. Foster, the retiring President, was delivered on Thursday evening in the First Presbyterian church. It was upon "The Latest Investigations in Geology and Archæology, with special reference to the condition of Pre-historic man."

Among the various objects of interest in and about Troy, none attracted more attention than the various Iron-works. The proprietors of the Burden, the Albany, and the Rensselaer Iron-works, and of the Bessemer Steel-works provided special facilities for their inspection. Mr. A. L. Holley, to whose mechanical skill the wonderful machinery of the latter establishment is due, was particularly attentive and courteous. The U. S. Arsenal at Watervliet, the Rensselaer Institute at Troy, and many other places of interest received also their share of attention.

Two excursions were made from Troy; one to Albany by invitation of the Albany Institute, the other to Saratoga by invitation of the citizens of Troy. The time in Albany was divided between the Dudley Observatory and the Geological Hall. A sumptuous entertainment at the State Library closed the day's festivities. The Saratoga excursion occupied an entire day, dinner being served at Congress Hall at 4 P. M. The Association was the recipient also of various private hospitalities.

The Association voted to hold its next meeting in Indianapolis, Indiana,—in accordance with an invitation from that city—on the 17th of August, 1871. A committee was also appointed to arrange for the following meeting in San Francisco in 1872, upon invita-

tion of the California Academy of Natural Sciences. The following officers were elected for the next meeting:

President, Prof. ASA GRAY, of Cambridge; Vice-President, Prof. George F. Barker, of New Haven; General Secretary, Prof. F. W. Putnam of Salem; Treasurer, Mr. Wm. S. Vaux, of Philadelphia.

The following is a list of the papers presented:

1. IN GENERAL SESSION.

1. On the Chemistry of the Bessemer Process; CLARENCE E. DUTTON.
2. Mrs. Willard's Theory of Circulation by Respiration; Mrs. A. L. PHELPS. (Read by J. G. Morris.)
3. Last Winter's occupation of Moosilauke Mountain in New Hampshire; J. H. HUNTINGTON.

2. IN SECTION A.

Mathematics, Physics, and Chemistry.

1. Some new applications of the Graphical Method; EDWARD C. PICKERING.
2. On Dispersion and the possibility of attaining perfect Achromatism; EDWARD C. PICKERING.
3. An Examination of the Doctrine of Atomicities; F. W. CLARKE.
4. The Isothermals of the Lake Region; A. WINCHELL.
5. A Description of a new apparatus for illustrating the Precession of the Equinoxes; JAS. BUSHEE.
6. The Magnetic Wells of Michigan; A. WINCHELL.
7. To whom is due the credit of the most important application of Steam as a Motive Power; CLINTON ROOSEVELT.
8. On Methods of illuminating optical Meteorology, particularly the formation of Halos and Coronæ. according to the theory of Bravais; JOSEPH LOVERING.
9. Researches in Electro-Magnetism; ALFRED M. MAYER.
10. Abstract of a research on a simple method of measuring Electrical Conductivities by means of two equal and opposed Magneto-electric Currents or Waves; ALFRED M. MAYER.
11. Unpublished experiments of Prof. W. B. Rogers on the Influence exerted by the presence of Carbonic Acid in gas, on its Illuminating Power; FREDERICK E. STIMPSON.
12. A Graphical Discussion of the various formulæ proposed for the relation between the quantity of light produced by the combustion of Illuminating Gas and the volume of gas consumed; FREDERICK E. STIMPSON.
13. Aurora Borealis; L. BRADLEY.
14. Cosmogony; L. BRADLEY.
15. The Northers of Texas; SOLOMON SIAS.
16. The Connection between Solar Spots, Terrestrial Magnetism, and the Aurora Borealis; ELIAS LOOMIS.
17. A Theory of the Constitution of the Corona of the Sun; SIMON NEWCOMB.
18. On the Assumption that Matter is Impenetrable; H. F. WALLING.
19. Elasticity, a mode of Motion: H. F. WALLING.
20. The conditions of Stable Equilibrium in Atomic Orbits: H. F. WALLING.
21. Spectroscopic examination of the Bessemer Flame; J. M. SILLIMAN.
22. Description of a new Meteorograph, for the automatic registration of Meteorological phenomena; G. W. HOUGH.
23. Remarks on the total fluctuation of the Barometric Column; G. W. HOUGH.
24. Relations existing between temperature, pressure, wind, and rain-fall, as indicated by automatic registering instruments; G. W. HOUGH.
25. On the rate of the Dudley Observatory Sidereal Clock for two years; G. W. HOUGH.
26. On a new form of mercurial horizon, by which vibrations are extinguished; J. H. LANE. (Presented by J. E. Hilgard).
27. Description of Batchelder's Arctic Tide-Gauge: J. E. HILGARD.
28. On proposed improvements for Common Roads; S. D. TILLMAN.
29. On a new Musical Notation; S. D. TILLMAN.

30. On improvements in Inland Navigation; S. D. TILLMAN.
31. On improved facilities for transmitting heat from one fluid to another; S. D. TILLMAN.
32. Gaseous and Liquid Rings; E. N. HORSFORD.
33. On the possibility of a Limit of Visible Magnitude; F. A. P. BARNARD.
34. On the brightness appearing on the limb of the Moon's Image, in Photographs of Solar Eclipses; F. A. P. BARNARD.
35. On the testimony of ancient Eclipses in regard to the Uniformity of the Earth's Rotation; J. N. STOCKWELL.
36. The Discovery of the force which originally imparted all their motions to all the stars; JACOB ENNIS.
37. On the Survey of the Northern Heavens instituted by the German Astronomical Society; T. H. SAFFORD.
38. On a New Method of determining the Level-error of the axis of a Transit instrument; C. A. YOUNG.
39. On Solar Prominences and Spots observed with the Spectroscope during the past year; C. A. YOUNG.
40. Some account of progress in the investigation of the laws of Winds; J. H. COFFIN.
41. Abstract of a paper on Temperature for twenty-five years; O. W. MORRIS.
42. The solvent power of anhydrous liquid ammonia; CHARLES A. SEELY. (Read by Prof. Walling).
43. The inadequacy of the prevailing Baconian system by Induction, and the fallacy of the too exclusive use of the *a priori* method; F. L. CAPEN.
44. On an improved form of Solar Eyepiece; S. P. LANGLEY.
45. The universal method of approximation; THOMAS HILL.
46. Note on the involute of a circle, and the analytical value of the hyperbolic base; THOMAS HILL.
47. Molecular classification; GEO. F. BARKER.
48. On the latest discoveries in regard to the manufacture of Ice by mechanical power; P. H. VANDER WEYDE.
49. Further improvements in the method of transmitting, audibly, musical melodies by the electric telegraph wire; P. H. VANDER WEYDE.

The following papers were read only by title:—

50. On Elasticity as a Feature in Physics; S. J. WALLACE.
51. On the Advancement of the Sciences; CLINTON ROOSEVELT.
52. On the present aspects of Organic Physics; HENRY HARTSHORNE.
53. Suggestions for systematizing chemical nomenclature; A. M. EDWARDS.
54. The relation between the bands of the Spectroscope and the musical scale; P. H. VANDER WEYDE.
55. The most important result obtained from the researches during some years of travel, consists in the establishment of the following theory:
The law of gravity is not the motive power of cosmic bodies; A. HABEL.
56. Gravity is not the principal motive power on terrestrial bodies, but acting very secondary; A. HABEL.
57. Past and Future Astronomy; JOSEPH TREAT.
58. Demonstration of the Perturbation of Uranus, which discovered Neptune; JOSEPH TREAT.
59. Universal Mathematical Demonstration of the impossibility of Gravity, or the Attraction of Matter as Matter; JOSEPH TREAT.
60. Corollaries of the Milky Way; JOSEPH TREAT.
61. An account of an Experiment upon the Physiological Action of Nitrous Oxyd; F. W. CLARKE.
62. Acid reaction of Tribasic Phosphate of Lime; E. N. HORSFORD.

3. IN SECTION B.

Geology and Natural History.

1. On the Early Stages of Discina; E. S. MORSE.
2. On the Organization of the Brachiopoda, (Discina and Lingula); E. S. MORSE.

3. The Brachiopoda as a Subdivision of the Annulata; E. S. MORSE.
4. Observations on Seedling Compass-plants (Silphium laciniatum); THOS. HILL.
5. The Development of Limulus Polyphemus; A. S. PACKARD, Jr.
6. The Terrace Epoch in Michigan; A. WINCHELL.
7. On the Relation of Organic Life of the several continents to the Physical Character of those land areas; N. S. SHALER.
8. On the character of the observations necessary to interpret the record of the last Glacial Period; N. S. SHALER.
9. On a method of collecting certain Geological facts adopted by the Social Science Association; N. S. SHALER.
10. Notes on the Condors and Humming birds of the Equatorial Andes; JAMES ORTON.
11. On the evidence of a Glacial Epoch at the Equator; JAMES ORTON.
12. On the Homologies of the Cranial Bones of the Primary Types of the Reptiles; EDWARD D. COPE.
13. On the Reptiles of the Triassic formations of the United States; EDWARD D. COPE.
14. On the existence of two classes of male flowers in the common sweet chestnut, and the influence of nutrition on the sex; THOMAS MEEHAN.
15. On objections to Darwin's theory of Fertilization through Insect agency; THOMAS MEEHAN.
16. On the law of fasciated branches, and its relation to the law of sex in plants; THOMAS MEEHAN.
17. The supposed Elevation and Depression of the Continent during the Glacial period; J. B. PERRY.
18. The Boulder-Trains of Berkshire county, Mass.; J. B. PERRY.
19. The Development and Old Age of the Tetrabranchiate Cephalopods; ALPHEUS HYATT.
20. The Genetic Relations of the Arietes; ALPHEUS HYATT.
21. The porphyries of Marblehead; ALPHEUS HYATT.
22. Geology and Topography of the White Mountains, N. H.; C. H. HITCHCOCK.
23. Description of a new Trilobite from New Jersey; C. H. HITCHCOCK.
24. The Distribution of Maritime Plants a proof of Oceanic Submergence in the Champlain Period; C. H. HITCHCOCK.
25. On the young of Orthagoriscus; F. W. PUTNAM.
26. On the salt deposits of Western Ontario; T. STERRY HUNT.
27. On Iron Sand Ores; T. STERRY HUNT.
28. Notes on Granitic Rocks; T. STERRY HUNT.
29. On the oil-bearing limestone of Chicago; T. STERRY HUNT.
30. On the Lignites of West America, their Distribution and Economic Value; J. S. NEWBERRY.
31. On the Sequence and Chronology of the Drift Phenomena in the Mississippi Valley; J. S. NEWBERRY.
32. On some new relics and traces of the Mound Builders; J. S. NEWBERRY.
33. On the relation of the Onedonta Sandstone and Montrose Sandstone of Vanuxem to the Hamilton and Chemung Groups; JAMES HALL.
34. Notice of the Fossil Plants of the Hamilton and Chemung Groups, with reference to the source of the sediments of these Formations; JAMES HALL.
35. Note upon the Rocks of the Huronian System on the Peninsula of Michigan; JAMES HALL.
36. Remarks on the occurrence of the genus *Dithyrocaris* in the Hamilton and Chemung Rocks of New York; JAMES HALL.
37. On the Geology of the Delta, and the Mudlumps of the Passes of the Mississippi. I. Geological Structure of the upper Delta plain. II. The lower Delta, and the Mudlumps of the Passes; E. W. HILGARD. (Read by J. R. Walker.)
38. Apatite Deposits of Lanark Co., Ontario, Canada; GORDON BROOME.
39. On some new generic forms of Brachiopoda, with remarks on some points of their structure; W. H. DALL.
40. On the order Docoglossa of Troschel; W. H. DALL.
41. On the nature of the foliage of Pines, etc.; a criticism; A. GRAY.
42. On the local Glaciers of the White Mountains; L. AGASSIZ. (Read by J. B. Perry.)

43. Notes on the relative age of the Niagara and so-called Lower Helderberg series; A. H. WORTHEN.
44. Probable Origin of the South Carolina Phosphates; W. C. KERR.
45. On some points in the Stratigraphy and surface Geology of North Carolina; W. C. KERR.
46. A point in Dynamical Geology; W. C. KERR.
47. Lava-ducts in Washington Territory; R. W. RAYMOND.
48. The Salt Marsh at Silver Peak, Southern Nevada; R. W. RAYMOND.
49. On Abnormal Vertebræ of the Elephant; SANBORN TENNEY.
50. On a new locality of Kyanite; SANBORN TENNEY.
51. On some points in the Geology of Eastern Mass.; SANBORN TENNEY.
52. Brief Notices on Hoosac Mountain and Tunnel; JAMES HYATT.
53. On the Relations of the orders of Mammals; THEODORE GILL.
54. On the subdivisions of the Branch Mollusca; THEODORE GILL.

The following papers were read by title only:—

55. Sketch of the researches made during seven years' travel through the five states of Central America, part of New Granada, the Republic of Ecuador and Peru; then on the Island of Chincha, and the Galapagos Archipelago; M. A. HABEL.
56. On the occurrence of native iron, not meteoric; H. B. NASON.
57. On the composition of the American Opium; H. B. NASON.
58. On parallel striæ in quartz crystals; L. FEUCHTWANGER.
59. Geology of the Cottonwoods Mining District of the Utah Territory; P. A CHADBOURNE.
60. The Geysers of Iceland and California; P. A. CHADBOURNE.
61. Guano, the origin of the Apatite of Rideau, Canada; E. N. HORSFORD.
62. Fresh Water Pond Overlying a Salt Water Pond in Middlesex county, Mass.; E. N. HORSFORD.
63. Evidence of glacial action in the placer and gulch gold of California; E. N. HORSFORD.
64. Lakes and Lake Regions; S. J. WALLACE.

4. IN SUB-SECTION C.

Microscopy.

1. Microscopic Circuits of Generation: *a.* Of Zymotic Fungus; *b.* Of the (nominal) Genera of Fresh Water Algæ, as development-phases of Bryaceæ, etc.; *c.* Of Vorticello-Planarians; T. C. HILGARD. (Read by J. E. Hilgard.)
2. On a new form of Binocular Microscope; F. A. P. BARNARD.
3. On the Structure of the scale of Podura plumbea; F. A. P. BARNARD.
4. On the Illumination of Binocular Microscopes, with proposal for a new Diaphragm-stop; R. H. WARD.
5. Remarks on Stereoscopic vision as applied to the Microscope; R. H. WARD.
6 Some remarks on Nobert's lines, with particular reference to Dr. Woodward's photographs; R. H. WARD.
7. Some remarks on a Pocket Microscope and Telescope combined; JOSIAH CURTIS.
8. Some remarks on two deposits of Diatomaceous earths recently thrown up by the Sea; EDWIN BICKNELL.
9. Remarks on a method of producing very low power with the microscope, with demonstration; EDWIN BICKNELL.

5. IN SUB-SECTION E.

Archæology and Ethnology.

1. Observations of the stone used by the Indians within the limits of Massachusetts in the manufacture of their implements, with some remarks on the process of manufacture; JAMES J. H. GREGORY.
2. The Substitution of General Laws for Special Legislation; GEO. A. LEAKIN.
3. On some peculiarities of the Grammar of the Eskimo Dialects; W. H. DALL.

4. Sign-Language as illustrative of the Laws of Written and Vocal Language; G. W. SAMSON.

5. Remarks on customs of the Waribiara and Blanco Indians in the vicinity of Chiriqui Lagoon, Central America; J. A. MCNIEL.

2. *On "Pure Scarlet;"* in a letter from WM. H. DALL (letter to Prof. Silliman dated Smithsonian Inst., Washington, D. C., May 3, 1870.)—In 1863, being engaged in making anatomical drawings from living microscopic animals, I purchased among other colors, a cake made by Windsor and Newton of England stamped with the name "Pure Scarlet." It was of the finest and most vivid scarlet, far superior in brilliancy to any of the various shades of vermillion, &c., which are commonly sold in the shops.

I had, however, no occasion to use the color until 1865, when I colored a lithographic plate (on ordinary paper used for such purposes) of *Pentstemon Cerrosensis* Kellogg, putting in the darker shades with carmine.

In the summer of 1865, at Sitka, Alaska Terr., I made some drawings of Æolis and other nudibranchiate mollusks, which had streaks of bright scarlet upon various parts of their bodies and tentaculæ. These were sent by mail to the Smithsonian Institution where they remained until my return from the north in 1868.

In 1866 being in San Francisco I chanced to examine the plate which I had colored the winter before, and noticed a fading of the colors, but not considering the matter particularly, I recolored the plate with the same pigment and left it in perfect condition.

Having returned to Washington, during the spring of 1869, I used the cake of "pure scarlet" again, for coloring a map (upon stout, thin, map-paper) to show the distribution of the Indian tribes. The color was laid on thickly and uniformly over the entire surface of two or three of the divisions of the map. As this lay on my table for convenient reference I noticed that it began to fade rapidly, and in the course of a few months the paper was left perfectly white and clean, not a trace of the heavy, opaque pigment remaining. This recalled the fading of the colored plate to my mind, and upon examination I found that the latter was in the same condition, not a particle of scarlet remaining, and the only color upon the plate being the streaks of carmine which had been put on by way of shading and which were unchanged.

I had believed the drawing of the nudibranchs to have been lost, as I could find it nowhere, but now a more careful search revealed the fact that the scarlet having entirely faded away, I had failed to recognize it. It was made on the best rough drawing paper of English make.

An examination of the substance showed it to be soluble in iodid of ammonium and hyposulph. soda, and with nitrate of silver in solution it gave iodid of silver, hence I suppose it to consist wholly or in part of iodid of mercury. I enclose a fragment for examination. The supposition that it was acted upon by hyposulph. soda in the paper is doubtful, in view of the fact that three kinds of paper, one of them made expressly for water color drawings, gave the same results.

As the pigment is an exceedingly attractive one, liable to be used by naturalists in valuable drawings, it may not be out of place to call their attention to its ephemeral properties.*

3. *Professor Marsh's Rocky Mountain Expedition. Discovery of the Mauvaises Terres formation in Colorado.*—Prof. Marsh and party left New Haven on the 30th of June for Omaha and beyond. By a letter from him dated Cheyenne, August 4th, we learn that he had already made a successful expedition to the Loup Fork, and obtained large collections of specimens, but they were prevented by the Indians from going on to the Niobrara region. The following is the copy of a later letter to J. D. Dana, dated Pine Bluffs, Wyoming Territory, Aug. 12th:

The Scientific Expedition from Yale College, while recently examining the geology of Northern Colorado, discovered an extensive outcrop of the true Mauvaises Terres or White River formation, at a point nearly 200 miles south of the region where it had been previously identified. The locality first detected, which contained all of the characteristic fossils of this deposit, was on one of the branches of the Little Crow Creek, about five miles south of the Wyoming State line. The strata there observed consisted of at least 150 feet of light-colored clays, overlaid by sandstones and conglomerates about 200 feet in thickness. The lower portions of the clays are the true Titanotherium beds, containing many remains of *Titanotherium Proutii*. Above these are similar clay deposits corresponding closely in age with those on the White River, and marked by abundant remains of *Oreodon Culbertsoni*, *Testudo Nebrascensis*, *Helix Leidyana*, and many other fossils, which characterize that horizon. Associated with these were found several new species of mammals and birds.

This interesting series of fresh-water Tertiary strata lies almost horizontal, dipping apparently, but very slightly, toward the northeast. It probably forms the southwest border of the great Miocene lake-basin, east of the Rocky Mountains, which is so remarkable for its extinct animal remains. Our party traced the same formation, with its more common fossils, about thirty miles northeast into Wyoming, along the hills known as Chalk Bluffs, and still farther north in the Pine Bluff range. We hope soon to examine it at other points.

4. *Glaciers of Scotland.*—Prof. Croll, in a memoir on the "Boulder-clay of Caithness," in the Geological Magazine for June, 1870, takes the ground that the boulder drift of Scotland is due mainly to glaciers, and not to icebergs, and points out the direction of the movement of the great glacier, not only over Scotland, but over the seas north and east, illustrating the subject by a map. He finds an argument for its glacier origin in the fact that the drift is *unstratified*, remarking that depositions from icebergs would necessarily be more or less stratified by the waters in which they fall. The argument is one that cannot be set aside,

* The color sent by Mr. Dall is, as he supposes, *mercuric iodid*, well known to chemists as very volatile, changing at a gentle heat from scarlet to nearly white, and the latter by friction returning again to scarlet.—S.

and holds as well for New England and the rest of North America as for Scotland. Prof. Dana, in his recent paper on New Haven Geology, draws the same distinction between the drift that was deposited by glaciers over the land, and by glaciers over waters.

Dr. Robert Brown also presents the glacier theory for Scotland in an article read before the Geological Society of London on June 22, entitled—On the Physics of Arctic Ice as explanatory of the Glacial remains in Scotland.

5. *Meteoric Irons.*—The Museum of Vienna has recently received an iron meteorite from Atacama having a maximum diameter of a third of a meter, and weighing 51 kilograms. The iron contains about 6 per cent of nickel.

Tschermak has studied the meteoric iron which fell at Lodran, near Moultan in India, on the 1st of October, 1868, a fragment of which was sent to the Vienna museum by Mr. Oldham. Niccoliferous iron constitutes about 32 per cent of it; and but for the iron, the meteorite is identical with terrestrial chrysolite rocks. It contains crystals of bronzite and bluish-gray chrysolite, and of magnetic pyrites and chromic iron. The chrysolite contains 12 per cent of protoxyd of iron.—*L'Institut*, July 20.

6. *Fall of a Meteorite in Stewart Co., Georgia.*—Prof. J. Lawrence Smith states in a letter to Prof. Brush, dated Louisville, Ky., July 18th, 1870, that a meteoric stone fell in Stewart county, Georgia, in October, 1869. He adds that a description of its fall and phenomena by Prof. Willet, and an analysis of it by himself, will be sent for the next number of this Journal.

7. Prof. Helmholtz takes the chair of Physics at Berlin made vacant by the death of Magnus. He will enter upon his new duties in April next.—*Nature*, July 28.

Prof. J. Watson has been awarded, by the Paris Academy of Sciences, the Astronomical Prize, Lalande foundation, for the discovery of eight new asteroids in one year.

8. *On the occurrence of a Peat bed beneath deposits of Drift in Southwestern Ohio.*—The author of the article on this subject, at p. 54, is Professor Edward Orton.

9. *Woodward on the Magnesium and Electric Lights for Photo-micrography.*—The illustrations of this article, on pages 298 and 301, of the last volume of this Journal, should be transposed.

OBITUARY.

Jacob P. Giraud, Jr., the ornithologist, died at Poughkeepsie on Tuesday, July 19th, in the fifty-ninth year of his age. A particulr friend of Audubon, Bachman and Wilson, and a member of the New York Lyceum of Natural History since 1840, he made his favorite science of ornithology his special study. In 1841 he published "A Description of Sixteen New Species of Texas Birds," and in 1844, a more valuable work on "The Birds of Long Island." His collection of North American Birds, one of the most complete in the country and containing many of Audubon's types, he presented to Vassar College. In addition he bequeathed to Vassar

College the sum of thirty thousand dollars to endow the professorship of Natural History, and two thousand dollars to complete his cabinet of North American Birds.

Mr. A. H. HALIDAY, of Antrim, Ireland, an entomologist of distinction died on the 13th of July, near Lucca, Italy. He graduated at Trinity College, Dublin, in 1822, at the age of 15, and five years afterwards as a gold medalist.—*Nature*, July 21.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *The Andes and the Amazon, or across the Continent of South America;* by JAMES ORTON, Prof. Nat. Hist. in Vassar Coll., Poughkeepsie. 356 pp. 12mo. New York, 1870. (Harper & Brothers).—Prof. Orton made his journey through South America in the years 1867–68, under the auspices of the Smithsonian Institution, with Messrs. P. V. Myers and A. Bushnell of Williams College, and others. His course from Paita, Peru, was by Guayaquil to Quito, and thence over the mountains to Napo; thence along the Napo river to the Amazon and Para at its mouth. The book of travels which he has given to the public is pleasantly written, and is full of facts and animated descriptions relating to the interesting region passed through—its general features and people, its climate and resources, its mountains and volcanoes, its landscapes and tropical productions. It contains a map of the part of South America from the Pacific to the Atlantic in the latitudes of the Amazon and its tributaries, which appears to have been prepared with care, and also several excellent woodcuts of scenery, etc. The party had not the time or instruments for extended physical investigations. Some of the observations of Prof. Orton have appeared in this Journal (vol. xlvii). Specimens were collected in zoology and botany, and some of fossils, and these have been put into the hands of different naturalists for description. Among the geological facts we observe the discovery of tertiary fossils at Pebas, on the Amazon, in long. 72° W., at a height of 345 feet above the sea; the fossils have been described by Mr. Gabb in the American Journal of Conchology.

2. *The Chemical History of the Six Days of Creation;* by JOHN PHIN, C.E. 96 pp. 12mo. New York, 1870.—This work is not, what its title seems to imply, a discussion by a chemist of the chemical history of the earth's genesis. It treats briefly of the bearing of some of the general principles of science, chemical, physical, geological and astronomical, on the interpretation of the first chapter of Genesis. The aim of the work is good. The views with regard to the "works" of the successive days accord in the main with those that have been presented by Prof. Guyot, and may be read with profit. There are also ideas peculiar to the author which we cannot commend. For example, he says that the "evening" of the "second day" resulted "from the cooling of the ashes of the great combustion;" that the "evening" of the "fifth day" resulted from the separation of Venus from the sun;

and the "morning" from "the restoration of the brilliancy of the sun;" the "evening" of the sixth day "from the separation of Mercury from the sun," etc. The author by bringing more exact science to bear on the subject and a wider philosophy will be enabled to prune his work of some manifest defects.

3. *The American Chemist: a Monthly Journal of Theoretical, Analytical and Technical Chemistry.* Edited by CHAS. F. CHANDLER, PH.D., and W. H. CHANDLER. New York, 1870. (William Baldwin & Co., 434 Broome St.)—The American reprint of the Chemical News having been given up, the subscription-list and stock of that reprint have been purchased by Mr. Baldwin, who starts the American Chemist to take its place, putting the editorial charge into the hands of Prof. Chandler and his brother. While we regret that a journal of the recognized ability of the Chemical News cannot be continued in this country, we believe that the well-known scientific reputation of the editors of the American Chemist is a guarantee that their new enterprise will not be second to it in value. The first two numbers have been received; i. e., those for July and August. They contain 32 articles, of which 17 are taken from the Chemical News, and eight are original. For the present, the admirable "Chemical Notices from Foreign Sources" printed in the Chemical News, will be retained. It is announced that after the close of the current year, the American Chemist will be conducted as an entirely independent journal. The subscription price is $5.00.

4. *On the Gulf Stream, and the Thermometric knowledge of the Atlantic Ocean and adjoining lands in the year* 1870; by Dr. A. PETERMANN. 64 pp. 4to, with three large colored temperature charts. Gotha, June, 1869. (Justus Perthes. From Petermann's Geogr. Mittheilungen, 1870, Heft 6–7.)—Dr. Petermann has here given an admirable exposition of the former and present state of knowledge of the temperature of the Atlantic and the Arctic seas and the lands adjoining, including the later results of the most recent expeditions. Deep sea temperatures, as well as the saltness of the ocean, and much detail with regard to special localities, come within the wide range of subjects under review. The first two charts contain the isotherms for every 2° Reaumur for the region between the meridians of 75° W. and 60° E., north of the parallel of 35°; the first, the isotherm for July and the second for January; and the third includes 5 charts of the arctic regions, giving the isotherms severally for January, July, the winter, the summer, and the isotherms of absolute minima for every 10° R. Eight pages are devoted to the instructions given to the second German North-polar Expedition, 1869–1870.

5. *Roasting of Gold and Silver Ores, and extraction of their respective metals without Quicksilver;* by G. KUSTEL. 145 pp. 12mo. San Francisco, 1870. (Dewey & Co.)—This little treatise contains an introductory chapter on the classification of silver ores, which is followed by a chapter on *roasting*, divided into sections, treating, respectively, of the chloridizing and the oxyd-

izing roasting of silver ores. The author then proceeds to give the different methods for the extraction of silver by lixiviation and subsequent precipitation; and, finally, a brief chapter on the extraction of gold by the chlorination process. The book contains a large amount of valuable facts expressed in a very compact manner, besides embracing much original matter, the results of the experience of the author and his friends in the Pacific States and in Mexico. It forms an excellent supplement to Mr. Kustel's previous work on the "Nevada and California processes of Silver and Gold extraction."

Bulletin of the National Association of Wool Manufacturers, founded Nov. 30, 1864. No. 1 of this Quarterly Bulletin appeared on January of 1869, and the first number of vol. ii in Jan. 1870. The numbers contain about 100 pages.

Report on the Coals and Iron Ores of Pictou County, Nova Scotia; by Edward Hartley, F.G.S., Mining Engineer to the Geological Survey; being an Appendix to Reports on the Pictou Coal-Field. From the Report of the Geological Survey of the Dominion of Canada, for 1867–69. 78 pp. 8vo. Montreal, 1870.

Index to the Literature of Uranium, by H. Carrington Bolton. Ann. Lyc., New York, Feb. 1870., pp. 362-377.

Uebersicht der im Königreiche Sachsen zur Chausseeunterhaltung verwendeten Steinarten; zusammengestellt von Dr. H. B. GEINITZ, K. Professor, and C. TH. SORGE. K. Oberbaurath. 116 pp. 4to. Dresden, 1870.

Paris Universal Exposition.—The following additions have been made to the list of Publications of Reports connected with the Paris Universal Exposition, given on page 258 of the last volume of this Journal:

Extracts from the Report of the International Committee on Weights, Measures and Coins, with a notice of the use of the Metric System in the United States, and its relations to other systems of Weights and Measures.

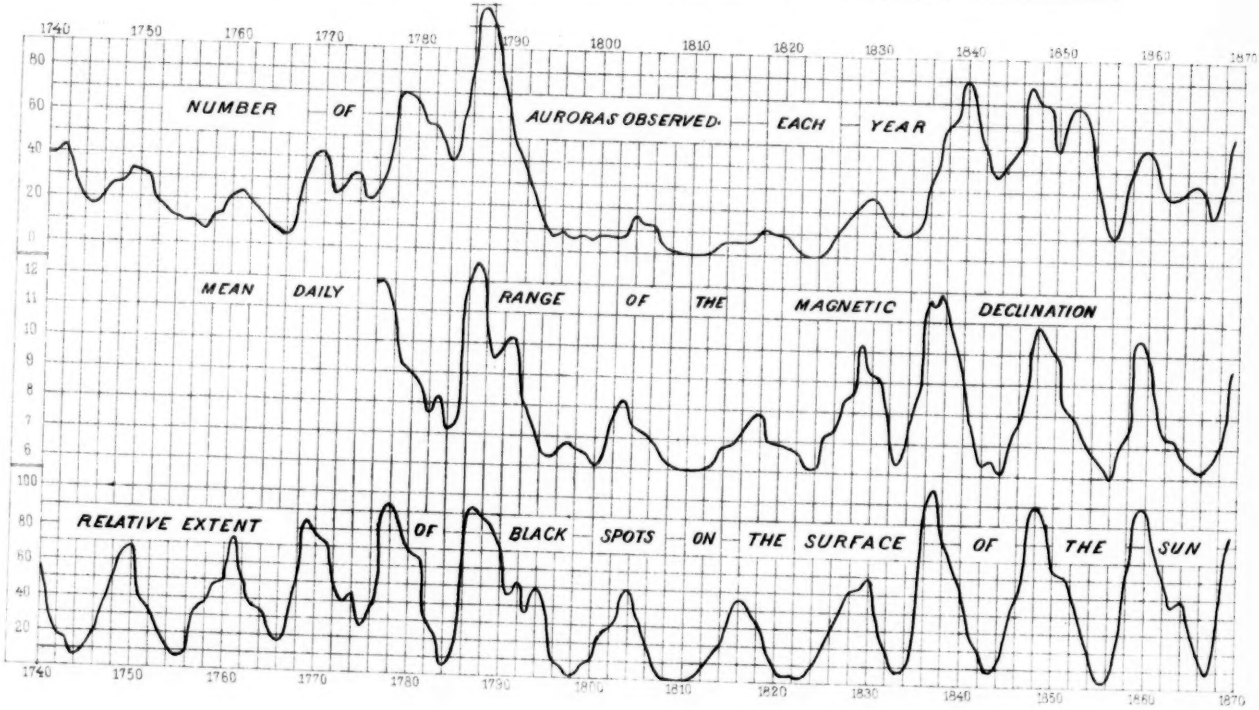
Report on Clothing and Woven Fabrics, being Classes Twenty-seven to Thirty-nine of Group Four; by Paran Stevens, U. S. Commissioner. 86 pp. 8vo.

Report on Education; by J. W. Hoyt, U. S. Commissioner. 398 pp. 8vo.

Bibliography of the Paris Universal Exposition of 1867; by Wm. P. Blake. 40 pp. 8vo.

Introduction, with selections from the Correspondence of Commissioner-General Beckwith and others, showing the organization and administration of the United States Section.

PROCEEDINGS OF THE ACAD. NAT. SCI. PHILADELPHIA, No. 1, 1870.—Page 1, Supposed Sivatherium from Colorado; *J. Leidy.*—p. 3, 4, 5, Reptile remains from Colorado; the Alabama Cretaceous; Fort Bridger Tertiary (a turtle named Baptemys Wyomingensis); *J. Leidy.*—p. 5 and 11, Fossil Birds of the U. S. Cretaceous and Tertiary; *O. C. Marsh.*—p. 6, Analysis of the nickel ore of the Gap mine, Lancaster Co., Pa.; *Wharton.*—p. 8, Mylodon from Central America; *J. Leidy.*—p. 9, Note on the Triassic Dromatherium sylvestre of Emmons; *id.*—p. 9, Note on Elasmosaurus; *id.*—p. 11, Dicotyles antiquus, from N. J. Miocene; *O. C. Marsh.*—p. 12, Ichthyodorulites; *J. Leidy.*—p. 13, Megalonyx Jeffersoni and Bison antiquus in Illinois; *id.*—p. 13, Growth of wood in Yucca: *J. Meehan.*—p. 14 Cross fertilization and the law of sex in Euphorbia; *id.*—p. 15 Relations of Synocladia of King to Septopora of Prout; *Meek* and *Worthen.*—p. 18, On Discosaurus and its allies; *J. Leidy.*—p. 22, New fossils from Illinois; *Meek* and *Worthen.*—p. 56, New fossils collected by the U. S. Coast Survey under Clarence King; *F. B. Meek.*

PROCEEDINGS OF THE CALIFORNIA ACAD. SCI., Vol. IV, Part ii, 1870.—Page 48, Discovery of a nearly entire skeleton of a Mastodon near Petaluna.—p. 50, Shells of Antioch. Cal.; *H. P. Carlton.*—p. 57, Shells of Truckee River and vicinity; *id.*—p. 61, Fauna of California and its geographical distribution; *J. G. Cooper.*—p. 82, 83, Indian mounds.—p. 86, Shell mounds; *L. Ransom.*—p. 88, Section of rocks in Hamilton, Nevada; *J. G. Clayton.*—p. 89. Rise of water in Mono Lake.—p. 90, Explorations in the Rocky Mountains: *J. D. Whitney.*—p. 92, the West-coast Fresh-water Univalves, No. I; *J. G. Cooper.*

SOLAR SPOTS, MAGNETIC DECLINATION, AND AURORAL DISPLAYS COMPARED.